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Antiquity

A QUARTERLY REVIEW OF ARCHAEOLOGY



Edited by Glyn Daniel

Vol. XXXIII No. 130 JUNE 1959

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Antiquity

VOL. XXXIII No. 130

JUNE 1959

Editorial

AS this June 1959 number of *ANTIQUITY* is published it is almost exactly a hundred years since belief in the great antiquity of man was publicly declared in the rooms in Burlington House of the Royal Society and the Society of Antiquaries of London. On the 26 May, 1859, Prestwich read a paper to the Royal Society entitled *On the Occurrence of Flint Implements, associated with the Remains of Animals of Extinct Species in beds of a late Geological Period at Amiens and Abbeville and in England at Hoxne*. Prestwich showed how the work of William Pengelly in Devon and Boucher de Perthes in north France had convinced him of the great antiquity of man. 'It was not,' he said, 'until I had myself witnessed the conditions under which these flint implements had been found at Brixham that I became fully impressed with the validity of the doubts thrown upon the previously prevailing opinions with respect to such remains in caves.' After Prestwich's paper, John Evans spoke about the flint implements themselves, and in her article 'Ninety Years Ago' published in this journal ten years ago, Dr Joan Evans reminded us of the circumstances of that meeting and of other meetings in that *annus mirabilis*, as it has so often and so wisely been called (*ANTIQUITY*, 1949, 122). John Evans wrote of Prestwich's paper that it 'would have been very good but he had only time to give an indifferent abstract of it and his voice was hardly audible in that room. . . . I had written an antiquarian letter to him to incorporate in his paper, and this he dexterously managed to leave behind him. The result was I had to stand up and give an extempore lecture. . . . There were a good many geological *nobs* there, Sir C. Lyell, Murchison, Huxley, Morris, Dr Perry, Faraday, Wheatstone, Babbage, etc., so I had a distinguished audience. Our assertions as to the finding of the weapons seemed to be believed.'

On 2 June, 1859, John Evans read a paper on the flint implements to the Society of Antiquaries in which he said, 'This much appears to be established beyond doubt, that in a period of antiquity remote beyond any of which we have hitherto found traces, this portion of the globe was peopled by men.' Both papers were well received: 'generally believed in', wrote Evans in his journal after the Antiquaries' meeting. In August of 1859 Sir Charles Lyell himself went to see the Abbeville pits, was convinced, and at Aberdeen on 18 September, in his Presidential Address to Section C of the British Association, and in the presence of Prince Albert as President, said that he was 'fully prepared to corroborate the conclusions . . . recently laid before the Royal Society by Mr Prestwich'.

The battle was won at last, as Dr Joan Evans wrote ten years ago, adding with good historical sense, 'It was an eventful week; the *Great Eastern* had just made its first Atlantic crossing; the men of the building trade had just struck for a nine-hours day; Brunel had died and Tennyson published the *Idylls of the King*' (*ANTIQUITY*, 1949, 123). It is useful and salutary, at this centenary moment, to look back at the nature and signifi-

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cance of the events of 1859. That same year, which saw the publication of Darwin's *Origin of Species* on 24 November, has brought forward in the last eighteen months a spate of lectures and books on Darwinism and Evolution. Not least among them, and most valuable to the archaeologist, is Loren Eiseley's *Darwin's Century* (London, Gollancz, 1959, 21s.). Dr Eiseley, who is on the staff of the University of Pennsylvania and is past president of the American Institute of Human Palaeontology, does not hesitate to quote, on his title page, Niebuhr's statement that 'He who calls what has vanished back again into being, enjoys a bliss like that of creating.'

The history of the study of antiquity is, in some ways, as important as its current development, and those who regard the present state of our knowledge in 1959 as an absolute, and not as a moment in the changing kaleidoscope of techniques, facts, interpretations and model-imposed prehistory, and who regard the study of 19th-century archaeology as nostalgia, are lacking in true perspective and wisdom. Camden referred to the 'back-looking curiosity' when he justified an interest in the study of antiquity; we are only beginning to realize that there are two aspects of this back-looking curiosity—the curiosity which drives us to study the remote past of man, and the curiosity which drives us to study how our predecessors studied their remote past. Sir Thomas Kendrick, this year retired from the Directorship of the British Museum, in his fascinating book *British Antiquity* (Methuen, 1950), gave us the story from Geoffrey of Monmouth to Camden. Professor Stuart Piggott in a series of articles and in his *William Stukeley* (Oxford, 1950) has done much to tell the story of the development of antiquarian study between Camden and Colt Hoare, and it is to be hoped that one day he will have the time and energy and inspiration to write for us a connected account of British Antiquity from the *Britannia* to *Ancient Wiltshire*. H. B. Walters's charming and stimulating *The English Antiquaries of the 16th, 17th and 18th Centuries*, published in 1934, is already a quarter of a century old, and was, even at the time, only an essay.

But while the English antiquaries of the 16th, 17th and 18th centuries have received attention, the history of archaeology in the 19th century has not yet received full and proper treatment. There have been, it is true, some initial and general studies like Michaelis's *A Century of Archaeological Discovery* and the present writer's *A Hundred Years of Archaeology*, but no one has, as yet, really given us an account of the development of archaeological ideas in the 19th century and related them to the development of Victorian thought. There is much to be done and many surprising discoveries to be made. Few people, for example, who read their Nilson and Worsaae at the present day can honestly say they are not surprised to find these books belong to the first half of the 19th century and not to the first decade of the 20th. What Worsaae, 'the first professional archaeologist in Europe', as he has been described, said about diffusion, independent evolution and cultural change before 1850 are now the commonplaces of popular books and lectures.

We have all been in the habit of thinking the Mesolithic a creation of the 20th century, and to look back at Allen Brown's use of it in the 1890s as a freak. But as Miss Judith Wilkins shows in her note printed below (p. 130) the idea of the Mesolithic had been set out by Hodder Westropp only a year after Lubbock had introduced the phrases Palaeolithic and Neolithic. Why was Westropp's idea of a Mesolithic not pursued? And, for that matter, why was Piette's idea not pursued; he had created a Metabatic Age—or Age of Transition—between what he called the Glyptique and the Neolithic? At present we can say that all these terms are a little ridiculous and almost as much fossil prehistoriography as the human fossils they describe are prehistory. But the evolution of ideas and concepts is so valuable to current scholarship if only in the deflation of complacency and smugness. We are no more at the height of prehistoric scholarship than were Thomsen, Worsaae, Gabriel de Mortillet,

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Pitt Rivers, Montelius, Sophus Muller, or Reginald Smith in their times. All the secrets of the prehistoric past are certainly not going to be revealed to the mid-20th-century generation, any more than they were to the mid-19th-century generation.

But that mid-19th-century generation—the generation of Layard, Rawlinson, Worsaae, Lubbock, and Daniel Wilson—did seem to pass through a more formative stage in the development of ideas than we are doing. To what extent was *The Origin of Species* one of those formative influences? It has been widely acclaimed as a major influence in the development of archaeology, but only, it seems to us, by those who have not really studied the development of archaeological ideas between 1810 and 1859. Darwin refused to discuss in *The Origin of Species* the relationship of evolution to man; he made this one cryptic statement on the general thesis in his book: 'Light will be thrown on the origin of man and his history', and, as Dr Eiseley has noticed, in later editions of the *Origin* the sentence begins 'Much light . . .'

It was not until 1871, in his *Descent of Man*, that Darwin really came out with his views on the relation of evolution to man, and by then T. H. Huxley's *Man's Place in Nature* had been published for eight years. The real value of Darwin to archaeology is that by his fine and reasoned argument he persuaded the world to accept the evolution of man. He was cautious, and Eiseley is right in admiring Darwin's caution in not involving man in the dispute until much later, in refusing to do what Lyell was so accustomed to saying, namely, 'going the whole orang' at one go. As far as archaeology was concerned, Charles Darwin was the impresario who made the discoveries already made, palatable, reasonable, serious. It was a great service.

But the discoveries had been made by others; by Boucher de Perthes in the Somme and Pengelly in Devon, by Thomsen and Worsaae in Denmark, and, in 1857, the first Neanderthal skull had been found. It is fascinating that Darwin actually read Boucher de Perthes and years later humbly confessed 'I . . . am ashamed to think that I concluded the whole was rubbish. Yet he has done for man something like what Agassiz did for glaciers.' It is, historically, of interest that, as a result of this, Boucher de Perthes's book is not among those which most affected Darwin; those were, as we are often being reminded at the present day, Malthus's *Essay on Population*, and Lyell's *Principles of Geology*. It is interesting to reflect at the present moment whether Darwin would have written a different *Origin of Species* if he had realized, as he clearly did not in 1859, the significance of the discoveries which were being made by Pengelly and Boucher de Perthes and Schmerling.

What, a hundred years from the *annus mirabilis*, is the most significant change in our knowledge of antiquity and our study of archaeology? It seems to us without any doubt not the accumulation of new facts like the knowledge of Palaeolithic art and the civilizations of the Sumerians, the Hittites, the Minoans, the Myceneans, and the Harappans, nor the development of scientific techniques of reconnaissance, excavation and interpretation, but the discovery of Carbon-14 dating. Here is something which Darwin and Huxley and Lubbock and Worsaae could not have thought of in their wildest dreams—that the antiquity of man could be exactly dated by scientific and non-archaeological means. Radio-carbon dating is the great revolution in 20th-century prehistory. The doubts that some have had about the validity of this technique, doubts that were summarized by Professor Milošević in his article in *Germania* (1957, 102-110) were dealt with, we had hoped, by Dr Barker in the December 1958 number of *ANTIQUITY*. But Professor Milošević writes to say that he is still dissatisfied and we will print later this year a short account from him of his doubts and dissatisfactions.

When all these are resolved we shall see in the next quarter-century the one thing that we are all waiting for—a complete and accurate dating of the main stages in the ancient

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development of man. We shall at last be able to write a truly historical—in the sense of a properly dated—prehistory of man. So important do American scientists regard radio-carbon dating that the *American Journal of Science* has announced the establishment of a *Radiocarbon Supplement* to be devoted wholly or largely to publication of radio-carbon date lists from laboratories in various parts of the world. Professors Richard Foster Flint and Edward S. Deevey, Jr., are the editors and the office of the *Supplement* is the same as that of the *Journal*, namely: Box 1905A, Yale Station, New Haven, Connecticut, U.S.A. Volume I of the *Supplement* appeared in May 1959. Thereafter, one volume will appear each year. The *Supplement* will be separate from the *American Journal of Science* and will be sent to a separate subscription list; the price of Volume I is \$2.50. We have already referred to the cards supplied by the Committee for Distribution of Radio-Carbon Dates (ANTIQUITY, 1958, 193). It looks as though we shall all have to have, in addition to our cards and needles from Andover, our *Supplements* from Yale. And, armed with these, we shall at last bring a true historical sense to our apperception of prehistory. ANTIQUITY will attempt, as the years go by, to inform its readers about the main significant changes in our appreciation of man's past that come from Carbon-14 dating. We have already presented the remarkable results of Dr Kathleen Kenyon's excavations at Jericho (ANTIQUITY, 1959, 5), and the necessary reinterpretation of New World Prehistory which radio-carbon dating involves (Chester Chard, ANTIQUITY, 1959, 44; Bushnell and McBurney, ANTIQUITY, 1959, 93). Now it is suggested by Dr Seton Lloyd in his report to the Ankara Institute that Anatolia may well have earlier evidence of village and city life than exists in what Breasted called the Fertile Crescent (*Daily Telegraph*, 28 February, 1959).

At this centenary moment, when we look back to Prestwich and Evans in Burlington House in 1859, we see the immense possibilities for European, Asian, American, and African—not to mention Australasian—prehistory by radio-carbon dating. We are entering a new era in prehistory, and many of us are forgetting the drama of this moment. Sir Thomas Browne, in his *Religio Medici* in 1635, said 'Time we may comprehend, 'tis but five days elder than ourselves, and hath the same Horoscope with the World.' Thomas Browne was of course thinking of antiquity in the chronological terms of his contemporary, Archbishop Ussher, who thought that the creation and man dated from 4004 B.C. Time, in the 17th century we *could* comprehend, and the creation of man on the sixth day *was* only five days younger than the world. Can we comprehend time and man's antiquity today, when we are told that the world is millions of years old and that Upper Palaeolithic cultures flourished 35,000 years ago, and that man himself has been in existence for three-quarters of a million years? With a great effort we can comprehend it, but it is a new horoscope of the world.

Surkh Kotal

by DANIEL SCHLUMBERGER

Professor Daniel Schlumberger, Director of the French Archaeological Delegation in Afghanistan, has excavated the extremely important site of Surkh Kotal in Bactria from 1952 to 1958, and here gives a summary of his work, already discussed in a preliminary way in the Journal Asiatique for 1952, 1954 and 1955, and in the Comptes-Rendus de l'Académie des Inscriptions, 1957.

I. THE SITE AND ITS DISCOVERY*

Until 1951, the great archaeological site we now call Surkh Kotal had completely escaped notice. In the autumn of that year, a friend of mine, Sarwar Nasher Khān, informed me that some stones bearing Greek letters had just been found in Northern Afghanistan by a team of workers engaged in building a new road. A few weeks later, we visited the site. It lay some 15 km. to the north-west of Pul-i Khumri, and some 12 km. to the south of Baghlān, two modern industrial centres in the valley of the Kunduz River. Having asked for the find spot, we were shown a ruined structure bordering the new road, at the bottom of a hill (henceforth called 'the acropolis') projecting like a promontory into the valley, and we could see at once that this structure was but a part of a large fortified enclosure of irregular shape following the contours of the hill-area. Inside this enclosure could be seen a smaller rectangular enclosure, the centre of which was occupied by a large flat-topped mound. Several architectural fragments were lying about. They were made of the local limestone. They included two big column-bases, and what appeared to be the remains of a mighty stele in alto-relievo, 2.20 m. high. Inquiring about the name of the place, we got several contradictory answers, two things only being clear: (1) that the place was a 'Kafir Kala', a 'Heathen's Castle'; and (2) that the saddle or pass connecting the hill with the mountains further west was called Surkh Kotal, 'The Red Pass'. In fact the ruin was anonymous, but 'Heathen's Castle, of the Red Pass' could be considered a suitable name. We shortened it into 'Surkh Kotal', 'The Red Pass'.¹

II. THE MONUMENTS UNCOVERED

Nine campaigns have taken place.² The first (1952) aimed at exploring the 'flat-topped

* The success of our excavations is very largely to be attributed to the friendly co-operation of the Afghan people. Were it not for an enterprising engineer who saved the inscribed stones when they were first discovered, or for Sarwar Nasher Khān, who reported the discovery, it is unlikely that we should ever have heard of Surkh Kotal. Without the enlightened care of their Excellencies Abdul Mejid Khān and Ali Ahmad Popal Khān, successive Ministers for Education, Isma'il Khān and Mohammad Juma Khān, successive Governor-Generals of Kataghān, without the active support of Ahmad Ali Khān Kohzad and Abdul Rahim Khān Zia'i, successive directors of the Department of Antiquities, nothing could ever have been achieved. Last but not least we always had with us a team of young assistants from the Kābul Museum, who were a great help.

¹ The antique name of the place, *Bagolango*, has been recognized by W. B. Henning (*BSOAS*, 1956, p. 366) in one of the first inscriptions, and his brilliant conjecture has been entirely confirmed by later discoveries.

² Preliminary reports of the first campaigns are to be found in the *Journal Asiatique*, 1952, pp. 433-53; 1954, pp. 161-205; 1955, pp. 269-79. The second of these reports includes the publication by M. Raoul Curiel of the epigraphic documents found up to 1953. A brief account of the fourth, fifth and sixth campaigns has been given

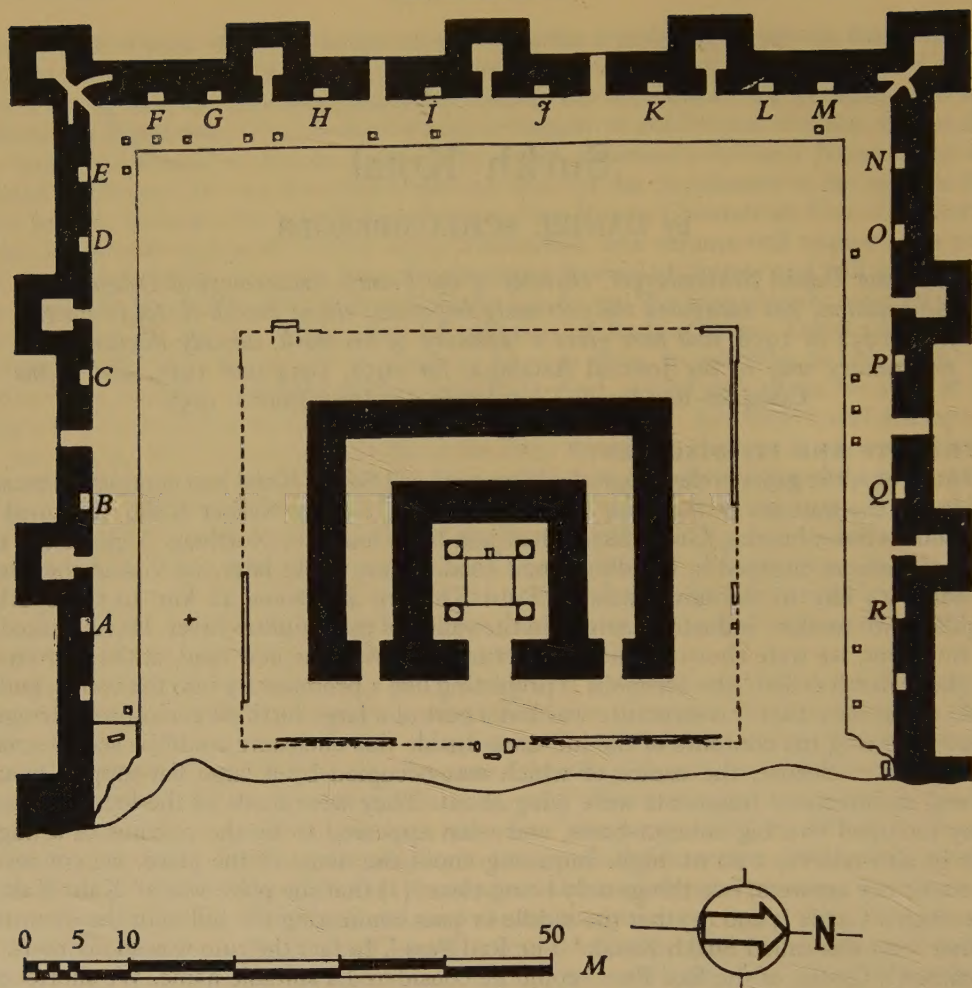


FIG. 1. Plan of the temple on the acropolis at Surkh Kotal with its colonnaded courtyard (1954). Later additions have been suppressed. The column-bases of the peristyle around the temple are not shown, none having been found *in situ*. Drawn by Marc Le Berre.

mound'. It yielded a temple (FIGS. 1, 2), the walls of which are built of mud-brick, reinforced with wooden beams, while some of its parts (the column-bases, the retaining walls) are made of stone. The plan is simple—a square room surrounded on three sides by a corridor. There are three doorways, all of them on the east side, the middle one leading into the square room, the other two leading into the corridor. The timber roof was supported by four columns, the bases of which still lay *in situ*. In the centre of the room there was a large square platform of fine ashlar masonry with three steps leading up to it (PLATE IX (a)).

in the *Comptes-Rendus de l'Académie des Inscriptions*, 1957 pp. 176–81. The publication by Mr André Maricq of the epigraphic documents found from 1954 to 1957, including the great inscription of Kanishka, is soon to appear in the same periodical. Short articles in English have been published in *Archaeology*, 6, pp. 232–8, and 8, pp. 82–7. The results of the ninth campaign, which has just ended (December 1958), have not been taken into account here; an unexpected new discovery has been made for which we cannot yet offer an explanation.

SURKH KOTAL

The building rested on a podium, and was surrounded by a peristyle, now destroyed, but the existence of which is quite certain from its foundations, which are still partly preserved.

The approximate date of the temple soon became clear when some coins were found: it belonged to the time of the Great Kushān Kings Kanishka and Huvishka. This was fully confirmed by all later finds, especially by the discovery in 1957 of a great inscription in Greek characters mentioning Kanishka, and likely to be the dedication of the sanctuary by that king. I shall abstain from any pronouncement about the precise date of Kanishka's accession, for this remains a much debated matter, recent opinions varying from A.D. 78 (Mrs van Lohuizen de Leeuw) to 144 (R. Ghirshman). As for the inscription, pending publication by Mr A. Maricq, I only want to say here that it is the first text of some length ever discovered in the East-Iranian language used by the Great Kushāns,³ a language

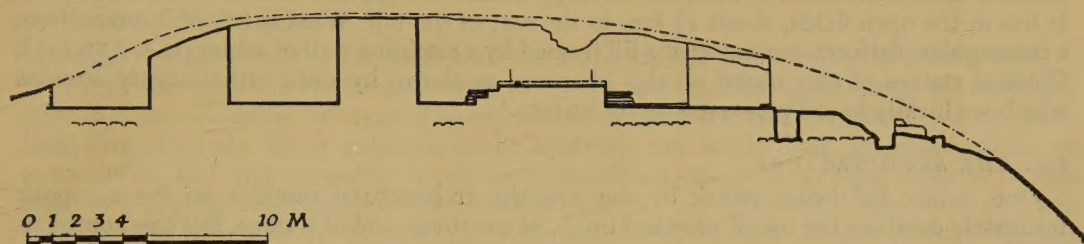


FIG. 2. West-east section across the temple (1952), showing the stone platform inside the cella, and two of the column-bases. On the rear (west) side of the temple the retaining wall of the podium is not shown, as it was still buried. *Drawn by Marc Le Berre.*

which we should call Tokharian, were not this name already busy designating another idiom.

The second campaign (1953) was devoted to excavating the courtyard around the temple, on the hilltop (FIG. 1). The clearing of the peribolos wall unexpectedly produced a secondary temple built against the outer face of the wall. An interesting feature of that small building was a square structure in the middle of the temple cella. The top of this solid mass of bricks, though half destroyed, still preserved a square hollow filled with grey ashes, showing it to be a fire-altar.

At the end of 1953 most of the peribolos wall had been cleared. From the outside, with its powerful square towers and its very narrow doorways, it looked more like a fortress than a temple-precinct. From the inside, however, the impression was different, the courtyard being surrounded by porticoes (PLATE IX (b)), the rear walls of which were adorned with great figures or groups of figures carved in clay and brightly painted, each of them set into a high rectangular niche. As there are at least eighteen niches (FIG. 1, A-R), and as each niche is likely to have contained more than one figure, there must have been scores of figures. Of all that sculpture only small fragments are preserved.

So far we had recovered the temple and the courtyard surrounding it on the hilltop, but the extent of the temple area on the hillside towards the East remained a matter for conjecture. In 1954 we began exploring the slope. Starting from the top, we dug at the rate of one campaign every year till 1956, and of two campaigns a year in 1957 and 1958. It soon became clear that the temple with its colonnaded courtyard formed but the crowning feature of a much vaster complex, the peribolos being found to extend down to the very foot

³ Up to now all the epigraphic fragments from Surkh Kotal are in that language.

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of the hill; the entire slope had been given architectural shape by scarping, four large terraces or tiers having been created, each of them 75 m. wide and limited towards the east by a mighty retaining wall, while the axis of the whole consisted in a monumental stairway leading up to the temple, each flight of steps resting on one of the terraces (PLATES X, XI).

The level reached today (autumn 1958) lies 67 m. lower than the hilltop, and the bottom still remains to be found. It seems very unlikely that we shall have to go down much deeper, but one important fact was revealed by our last campaign, and should be stressed: the foot of the hill lies hidden under heavy layers of accumulation, more than 4 m. deep, and the reconnoitring of this most important part of the sanctuary, including at it seems its monumental entrance, is scarcely begun.

Entirely apart from the great complex of buildings on the acropolis a small monument remains to be mentioned. It was discovered by chance in 1954 and excavated that same year. It lies in the open fields, about $1\frac{1}{2}$ km. to the east of the hill. What is left of it amounts to a rectangular platform consisting of a fill framed by a retaining wall of ashlar (PLATE XII (a)). Colossal statues of clay rested on this platform, as shown by some rather scanty remains which we luckily found preserved on its surface.⁴

III. THE ARCHITECTURE

One cannot fail being struck by the way the architectural remains on the acropolis intimately combine the use of unbaked brick, of cut stone, and of timber. But this combination loses much of its singularity if one recalls the palaces of the Achaemenids, which were built just like that. In spite of the distance, geographical as well as chronological, it seems we have here a survival of the great building tradition of the Persian empire.

Iranian, too, is the plan of the temple, as is sufficiently shown by one monument of similar outlay, formerly excavated by M. Dieulafoy near Susa. Iranian, we believe, was the cult. Dieulafoy thought he had discovered a fire temple. From the start we too held our main temple to be such a building (though not necessarily Zoroastrian, of course), the stone platform in the cella being best explained as a substructure for a fire altar. And the actual discovery of such an altar in the small temple we considered a confirmation of our conjecture.

Iranian, lastly, was at least some of the architectural decoration; for instance, the battlements, as shown by a number of stepped slabs, very typical indeed with their ornamental loopholes shaped like arrow-heads.

On the other hand, a number of features of both architecture and ornament are of unmistakable Greek origin, instances being the peristyle around the temple, the porticoes enclosing the courtyard, the Corinthian pilasters on the retaining walls of the podium, the attic profiles of the column bases, fragments of a frieze with garlands carried by Amorini, etc.

What we have, then, is a Greco-Iranian production, a monument made for an Iranian cult (whatever its precise nature) and mainly following Old-Persian traditions, although these now appear in Greek disguise. Let us stress that in spite of these Greek elements this architecture remains strikingly conservative. No signs are yet to be seen here of that spirit of innovation which is so notable a feature of the contemporary Greco-Iranian architecture of Mesopotamia, as at Hatra for instance, where stately iwāns were then being erected and boldly covered with magnificent barrel-vaults, or at Assur, where the use of sculptured

⁴ They must have been Buddhist figures. Proof of the Buddhist character of the platform is provided by one of the Corinthian capitals showing amidst its foliage a small turban on a draped socle; unquestionably the headgear of the Bodhisattva, see *Journal Asiatique*, 1955, p. 277, and now I. Lyons and H. Ingholt, *Gandhāran Art in Pakistan*, New York, 1957, p. 61, no. 50.

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stucco on façades was already starting the development which was to lead from late Hellenistic architectural ornament to Sassanian architectural tapestry. At Surkh Kotal we have no iwāns, no trace of vaulting of any kind (but ample proof that all the roofs were made of timber), no sign whatsoever of the use of stucco.

Last but not least, this architecture so far does not seem Indian, although many of its items have their counterparts in Gandhāra. But none of these are specifically Indian, all simply appear to be Greco-Iranian. This marks a great difference from Gandhāra itself, where truly Indian elements are of course common.

Here, then, is an art different both from the art of Parthian Mesopotamia and from the art of Kushān India, but nevertheless closely related to both of them. This is especially apparent from the sculpture.

IV. THE SCULPTURE

I shall restrict myself to the links with the art of India.⁵ They are of the closest kind.

On the one hand the Hellenizing items in the decoration of the great sanctuary recall the ones in Greco-Buddhist art to such an extent that nobody, one can rest assured, would have held these capitals, these garlands, to be anything but spoils from Buddhist stūpas or monasteries, had they turned up bit by bit in the Kābul bazaar, as so often happens, rather than in a regular excavation, as they luckily did. On the other hand, the sanctuary has yielded some sculpture in its own right: the stele in alto-relievo already mentioned, and three standing statues. Now, these pieces represent Kushān princes of the Kanishka dynasty (PLATE XII (b)) and are strikingly similar to the figures of Kushān princes discovered at Mathurā in 1911, one of which shows Kanishka himself, as an inscription tells us.

Usually, between the art of Gandhāra and the art of Mathurā contrasts rather than links are being stressed. Now at Surkh Kotal we are suddenly confronted with a new fact: there was an art of Kushān Bactria, and this art was intimately connected with both of the great schools of Kushān India. This has some bearing on the much debated problem of the origins of Greco-Buddhist art.

According to A. Foucher's traditional view, these origins were to be sought in the encounter of Greek art with Buddhist faith on North-West Indian soil, just before Greek domination over this country had ended (i.e. in Foucher's opinion, before 75 B.C.). As this view met with increasing difficulties, a new school of thought progressively evolved an entirely different explanation: that the so-called Greco-Buddhist art of Gandhāra was to be considered an offshoot not of Greco-Bactrian but of Greco-Roman art; that it was not a late and remote development of the Greek world, itself remote, that had flourished on the Oxus (and the existence of which ultimately was due to Alexander's military conquest), but a consequence of that brisk commercial intercourse by which products of the crafts of the Mediterranean, especially of Roman Egypt, had been spread all over India. Accordingly this art was to be renamed Romano-Buddhist; and many readers of *ANTIQUITY* will no doubt remember the uncommonly clear, skilful and brilliant defence of the new theory contributed by Sir Mortimer Wheeler to this periodical just ten years ago (*ANTIQUITY*, 1949, 4).

But the finds at Surkh Kotal are not easily to be reconciled with that doctrine. The use of the Greek alphabet by the Kushāns on their coins, being linked with its use by the Bactrian Greeks through an uninterrupted chain of issues as well as with its use by the Parthians through paleographic peculiarities, was from the start likely to be independent of Roman

⁵ The links with the art of Parthia are of great interest, but require a more detailed study than could be given in these short pages.

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influence. Now with this alphabet used on Bactrian soil for monumental inscriptions in non-Buddhist and probably pre-Buddhist buildings,⁶ the idea of such an influence at an early date becomes even more unlikely. Just the same, the princely statues at Surkh Kotal and at Mathurā recall, in motive as well as in style, certain Parthian figures, as some of the Gandhāra statues recall certain others⁷ without anything here being due to Roman art. Of course, the existence of a powerful trade linking the Roman empire with the Kushān world cannot be denied, attested as it is by so many finds, the most remarkable of all being the astonishing Begram trove; and the advocates of Romano-Buddhist art are very likely to be right when they consider that some Romanizing compositions in Gandhāra sculpture reflect these commercial ties, or that the stucco technique of later times was brought to India and Afghānistan from Roman Egypt. But the finds at Surkh Kotal as well as at some other places (Ayrtam on the Oxus, the Kuh-i Khwāja in Sistān), definitely point towards Gandhāra art having taken shape in a world still outside the reach of Roman influence, in a world of Hellenized art at Iranian courts.⁸ They point towards this art being originally nothing else than Greco-Iranian art made to serve Buddhism.

⁶ Whether the 'platform in the fields', the only Buddhist monument known so far at Surkh Kotal (above p. 84), should be considered contemporary with the ruins on the acropolis or somewhat later, remains a problem, but recent research (1958) rather tends to show it to be later.

⁷ To give just one instance, the bronze statue of Shami (Susiana), aptly compared by H. Seyrig with a statue of Palmyra (*Syria*, xx, 1939, p. 180; xxi, 1940, p. 332), is no less akin in style to some of the Buddhas of Gandhāra.

⁸ That great unity actually reaches much further than Gandhāra towards the East, much further than Mesopotamia towards the West; it extends from Palmyra to Mathurā, from the Syrian steppe to the plains of the Ganges, as is already shown with perfect clarity by H. Seyrig, 'Ornamenta Palmyrena Antiquiora', *Syria*, xii, 1940, pp. 277-328 (reprinted, with some corrections, in *Antiquités Syriennes*, iii, pp. 64-115).

Book Chronicle

We include here books which have been received for review, or books of importance not received for review, of which we have recently been informed. We welcome information about books, particularly in languages other than English and American, of interest to readers of ANTIQUITY. The listing of a book in this chronicle does not preclude its review in ANTIQUITY.

ABBEYS: An Introduction to the Religious Houses of England and Wales. By R. GILYARD-BEER. pp. 90, 28 text figures, 12 plates. *Her Majesty's Stationery Office, London*, 1958. 5s. A welcome addition to the publications prepared by the Ministry of Works; its purpose is to provide a background to the study of individual abbeys and priories in the care of the Ministry.

EXCAVATIONS IN SOUTHWARK. By DR KATHLEEN M. KENYON. pp. 112, 37 text figures, 8 plates. *Surrey Archaeological Society*, 1959. 30s. This, the fifth research paper of the Surrey Archaeological Society, describes the excavations in Southwark in 1945-7.

THE ANCIENT MARINERS. By LIONEL CASSON. pp. 286, 6 figures, 8 plates, 2 end-paper maps. *Gollancz, London*, 1959. 21s. An account of sailing and sea warfare in the Mediterranean and by Mediterranean peoples from the beginnings to the end of the Roman Empire by a Professor of Classics in New York University.

A FORGOTTEN KINGDOM. By SIR LEONARD WOOLLEY. pp. 208, 32 plates. *Max Parrish, London*, 1959. 30s. A new edition of a work first published as a *Pelican* in 1953.

[continued on p. 119.]

Flint Mining in Neolithic Denmark

by C. J. BECKER

In this article the Professor of Prehistoric Archaeology in the University of Copenhagen describes the discovery and exploration of flint mines in Denmark, unknown ten years ago.

THROUGHOUT the Neolithic Period the inhabitants of what nowadays is Denmark profited from the country's wealth of flint for implements and weapons. There were quantities of large and small nodules to be picked up along the shores, but even the most plentiful supplies were not always ample for their needs. In the early part of the Late Stone Age, when numbers of large stone axes were required for forest clearance, and at the close of the Neolithic Period when the idea was adopted of making daggers of extraordinary dimensions, while simultaneously a considerable export trade was started of implements to the flintless regions of the rest of Scandinavia,¹ surface flint no longer sufficed. There had also to be a rational exploitation of the occurrences of excellent pure flint in chalk deposits that were undisturbed and relatively easy of access.

Up to the year 1950 the belief in that possibility was pure theory.² But then it was that the first signs of Neolithic mining were discovered at Aalborg, in the North of Jutland (FIG. 1). The chalk around Aalborg (Senonian) contains little flint, but the Stone Age people had discovered a single seam of flint nodules, somewhat small in size, and they worked it over an extensive area.

Where the flint layer was just below the present surface of the ground they simply dug through the chalk in irregular trenches and pits; but as the flint seam did not lie parallel to the surface they were soon compelled to sink shafts. Three of these workings were unearthed by the National Museum excavations in 1950.³ The shaft in the best state of preservation measured 1.85 m. in diameter at the surface and just over 2 m. in depth. The shaft was funnel shaped above and widened out again towards the bottom, so that it had acquired the shape of a bell or beehive; for when the flint seam was reached the miners excavated as far out to all sides as they dared without danger of a subsidence. As a result, the bottom of the pit had a diameter of about 3 m.

Further investigation at the spot—by the Aalborg Historical Museum⁴—revealed larger and deeper shafts, but all sunk on the same principle (FIG. 2). The deepest measured 4.7 m., that evidently being the limit to which the miners had ventured to go. The flint seam was still of the same good quality, but an extension of the excavation area higher up the hill brought no more shafts to light.

Fifteen shafts were emptied in the course of the 1950–3 excavations; moreover, a large number were discovered, either in the course of building operations within the area or because their extension outwards at the bottom had broken through the walls into the already examined pits. This means that that particular area had been methodically ex-

¹ J. G. D. Clark, 'Objects of South Scandinavian Flint in the Northernmost Provinces of Norway, Sweden and Finland', *P. P. S.*, 1948, 219; C. J. Becker, *Acta Archaeologica*, XXIII (1925), 76.

² *Aarbøger f. nordisk Oldkyndighed*, 1947, 302.

³ C. J. Becker, 'Late-Neolithic Flint Mines at Aalborg', *Acta Archaeologica*, XXII (1951), 135.

⁴ S. Grantzau, 'Stone Age Mining', *Kuml* (1954), 30.



FIG. 1. Situation of Danish chalk and limestone deposits (in most cases overlain by glacial clay and sand), with Neolithic flint mines indicated: 1. Aalborg. 2. Hov. 3. Bjerre.

hausted of flint; the workings lie close together over a large area (at least 100×50 m., but probably much larger). Here and there, in half-filled pits or on the ancient ground surface, were small rough-dressing sites where the flints were chipped more or less into shape. Numerous abortive attempts show that daggers and sickles were the products there (axes quite exceptionally) in the Late Neolithic (about 1800 B.C.). No trace was found of any contemporary dwellings: the miners must have come there solely for the purpose of working the flint deposit, in the same professional manner as has been revealed by many British and Continental finds of industrial flint extraction.

And now discoveries have been made of earlier and more extensive flint workings. At Hov, Thisted county in Northwest Jutland (see map, FIG. 1), distinct remnants of ancient mining were observed in the wall of a modern chalk-pit. Three workings were examined in the course of the excavations of 1957 and 1958. Of the first, only the lower part remained in

FLINT MINING IN NEOLITHIC DENMARK

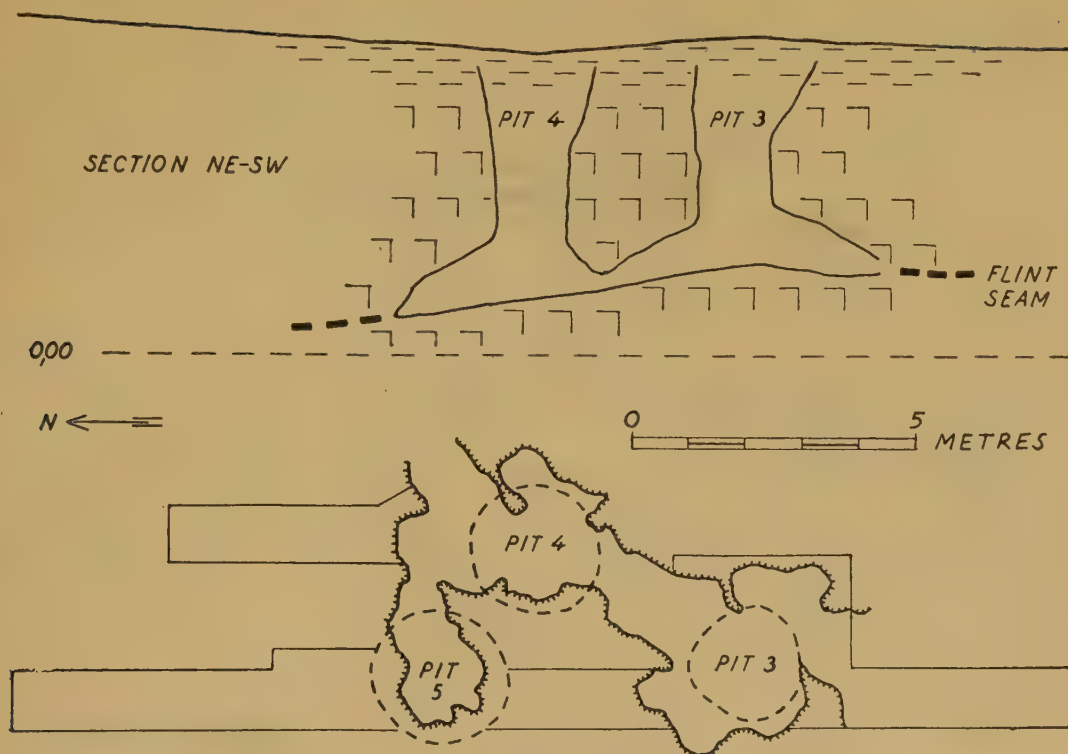


FIG. 2. Plan and section of Late Neolithic flint mines at Aalborg. The largest shaft is 4·7 m. deep (from survey records in Aalborg Museum).

the form of an almost cylindrical shaft, about 3 m. wide and carried 7 m. down into the chalk. Quite obviously the shaft was abandoned because of the poor quality flint there, for which reason neither lateral diggings nor drifts were to be seen.

There had been more success with the next shaft (FIGS. 3, 4). This too was circular, 4·5 m. wide at the top, only about 3 m. lower down. The depth was 8 m., but just at that level there had been no useful flint either. (Modern chalk quarrying showed that elsewhere at about the same depth was a seam of particularly good and large flint nodules, a fact which had evidently been known to the Stone Age people.) In this Shaft II, however, they had worked a seam of small nodules situated 2 or 3 m. higher up, having dug into the wall there and also excavated regular drifts. Two drifts started at the shaft, one 5 m. (PLATE XIII a), the other more than 3 m. long. At the shaft the longer one was so high that a man could work there upright, but farther in it became so low that the last flint would have to be extracted by the miner in a prone position. All the innermost branches were connected up with drifts or galleries from other shafts. In other words, here again it is evident that there were a number of shafts, sunk quite close to one another.

In September 1958 another shaft was emptied (Shaft V), 10 m. to the East of Shaft II. It was 5 m. wide at the top and resembled the others in being funnel-shaped downwards for the first 2 or 3 m., whereafter the walls were almost vertical with a diameter of about 2·5 m. The bottom was no more than 6·5 m. below the surface after the first bed of regular nodules

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FIG. 3. Plan of Early Neolithic flint mine at Hov (Shaft II).

was reached. In this particular spot, however, the flint had been too poor in quality, full of unsuspected cracks and cavities, so the Stone Age miners had abandoned it. For the same reason there was only one drift, unfinished, almost 3 m. long, 2 m. wide and of man's height, in the east wall near the bottom. Probably not a single satisfactory axe was gained from the mining of the entire Shaft V. It is imaginable that strong language was used while the pit was being filled in again.

Borings from the surface round about the excavated workings have determined the fact that there must have been at any rate twenty-five large shafts. There may have been many more, but the whole area has not yet been examined. In other words, this place too must have been the scene of a large-scale mining industry where the flint seams were worked systematically.

The fill from several of these ancient shafts contained worked flints, including a large number of unfinished thin-butted axes. So far, these represent the sole possibility of establishing an archaeological dating (Early Neolithic or Early Middle Neolithic, i.e. about 2500-2000 B.C.). Again, at Hov there was not the slightest trace of Neolithic habitation, or any finished implements, potsherds or the like in the shaft fillings.

FLINT MINING IN NEOLITHIC DENMARK

North

South

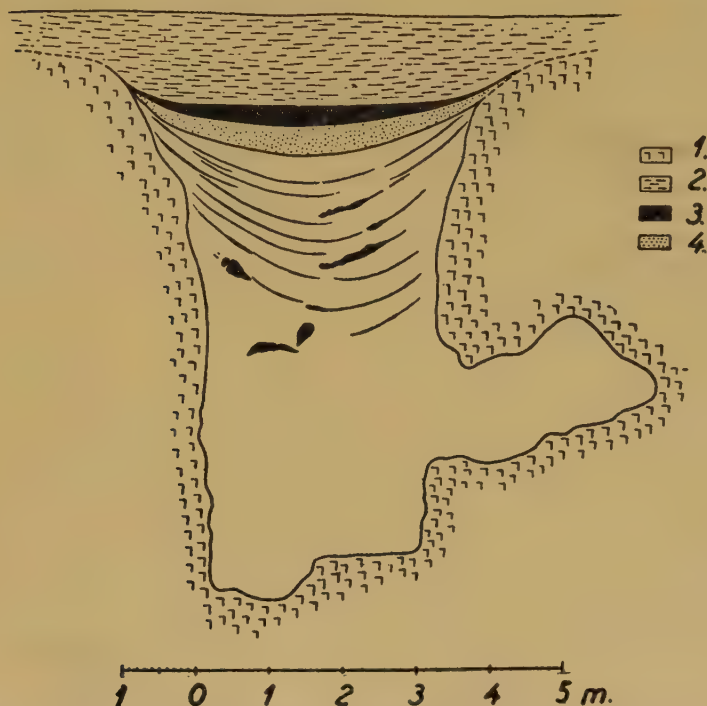


FIG. 4. Section of Early Neolithic flint mine at Hov (Shaft II). A drift, about 3 in. long, can be seen in the upper flint level. (1. Chalk *in situ*. 2. Modern soil. 3. Old soil. 4. Sand.)

One important observation was made at both Aalborg and Hov. Whenever a shaft was sunk in the Stone Age and then finished with, it was filled in immediately. No earth and sand had dribbled down to the bottom, and there were very few finds of the remains of mice or other small animals which had perished down there. At Hov there were none in Shaft I, and in Shaft II nothing but a toad (about a metre higher up than the bottom). However, in Shaft V there were eighteen mice and voles, but all at a level about 2 m. up from the bottom, at which stage there had evidently been a pause in the filling-up process. The succession observed in the fill suggests that the material was thrown back into the hole from which it had come; so far, the fill in the lower half of the shafts has consisted solely of chalk, whereas the upper half is a mixture of chalk and soil. If more than one shaft were open at a time the fill from the next one would surely have been thrown into the last one to be sunk, whereby the superficial layers would land at the bottom.

The flint nodules were given a preliminary shaping immediately they were brought up from below, which is why flint flakes, unfinished faulty axes and hammer-stones were mixed in with the upper fill in the shafts; and, as it looks as if each shaft had been filled in with the material taken from it, the inference is that the number and character of worked flints found provide a rather reliable index to what the ancient miners succeeded in extracting from each shaft. By this means it is confirmed that the unfinished Shafts I and V must

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have been highly disappointing, in as much as the miners failed to strike a single useful nodule. On the other hand, the same two shafts provide an idea of the strength of the organization behind all this activity; it was not discouraged by a few abortive workings, even if they meant weeks or rather months of futile hard labour.

Some information, though not at all exhaustive, could be gleaned of the technical methods of these Stone Age people at Hov. In the upper strata the chalk is somewhat loose and is so cracked that digging it out would not be much of a problem. Lower down, however, it becomes so hard and coherent that large and small blocks are still to be seen (sizes of about $25 \times 20 \times 20$ cms. are not uncommon). The walls still bear traces of the miners' tools in the form of round holes between 1 and 2 cm. in diameter. Apparently the tools were picks and wedges of antler, the same as have often been observed in flint workings abroad. But curiously enough not a single piece of antler has been found in any of the shafts so far examined, nor have the quarrymen of today seen any in those shafts which have been destroyed by modern chalk digging.

All three shafts showed traces of a horizontal platform, no doubt put up to facilitate disposal of the material dug out from below. In one-half of the wall impressions could still be seen of pointed poles, 8 to 12 cm. thick, which had been driven up to 40 cm. horizontally into the chalk (PLATE XIII *b*). In two of the shafts the platforms had been about 2 m. above the shaft bottom, whereas in the third one it was higher up. Another detail: In Shaft II it was possible to trace a 'post-hole', an impression of a slender tree stem, 10 to 12 cm. thick, whose side branches had been lopped off a short distance from the stem. The impressions of it could be followed at least 5 m. obliquely down into the shaft. This presumably was a climbing-pole, used by the Neolithic miners for getting in and out of the hole.

And most recently of all, in the summer of 1958 the existence of the third flint mine in Denmark was discovered. About 11 km. north-west of Hov, at Bjerre (not far from the large German Second World War fortress of Hansted), signs were found of several ancient excavations in chalk deposits of the same period as at Hov (Senonian); one of them was definitely ascertained to be a mine shaft of the same wide type as at Hov. This site has not yet been further examined, but the flint chips found make it seem likely that the material was required for flint celts.

Thus within the space of a few years it has been possible to demonstrate remains of systematic mining for flint in the Danish chalk beds too. With what we know from Great Britain, Belgium, France, Poland and other countries⁵ it would indeed have been remarkable if flint mines had not been discovered here. Today we know where to look for the old workings, but also that only few places in present-day Denmark presented chances of discovering the good flint in such quantities and under such conditions that industrial extraction would pay. It is astonishing that the Neolithic experts were able to pick these deposits out at a time when forest and scrub still covered a very large part of the country. On the other hand, it should be borne in mind that the Early Neolithic Period coincided with one of the great transgressions of the Litorina Sea, when the rising waters must have exposed and washed many slopes and cliffs composed of chalk.

⁵ J. G. D. Clark, *Prehistoric Europe, The Economic Basis* (1952), 174 ff. (with references).

New World Origins Seen from the Old World

by GEOFFREY BUSHNELL and CHARLES MCBURNEY

In this article Dr G. H. S. Bushnell, Curator of the Museum of Archaeology and Ethnology in the University of Cambridge, and his colleague Dr Charles McBurney, Lecturer in Archaeology in the University, discuss the problems of New World Origins in special relation to Wormington's Ancient Man in North America, the excavations in the Danger Cave, and the theories put forward by Chester S. Chard in his article in the last number of ANTIQUITY, 1959, 55.

THE last few years have seen a rapid advance in our knowledge of the early inhabitants of America, not of their bones, which are still elusive, but of the ways in which they lived and the things they made and used. The appearance of two important publications¹ gives occasion for a brief review of the present state of the problem and how it appears from outside.

Dr Wormington's book is a new edition of an old friend, which is, like its predecessors, indispensable to anyone who wants to keep up with the material. It gives concise factual accounts of a large number of finds; it aims at inclusiveness rather than selection, and the material is becoming rather overwhelming in volume, but the evidence, or lack of it, for dating a site is always clearly set out and the reader has no difficulty in picking out those which are really significant. Wormington can now divide her material into Palaeoeastern and Palaeowestern Traditions, of which more will be said below, as well as a Palaeonorthern one which has not yet been proved to be so early in date. Professor Jennings' monograph had not been published when her book appeared, but she had an idea of its significance from progress reports. It describes the excavation of three caves in western Utah, of which by far the most important is Danger Cave; the evidence is fully but rather naïvely set out, and it would have gained by being more concisely expressed.

We cannot forbear to utter a plaint about radio-carbon dates. It is time that archaeologists made up their minds what system they are going to adopt; Wormington gives them as B.P., and Jennings in terms of Christian chronology. Having become accustomed to think in terms of it, we much prefer the latter, and in any case the conversion has led each of our authors into at least one error (Jennings, p. 267, gives the Fort Rock sandals as 8100 B.C. instead of 7100 B.C., and Wormington, p. 148, has converted a figure of 6219 B.C. in the Modoc Rock Shelter into 7273 instead of 8173 B.P.).

A good deal has been known for some years about the hunting cultures of which the chief remains are found in the Great Plains. These make up what Dr Wormington calls the Palaeoeastern Tradition. The hunters have left traces of their presence in places where their prey was butchered, and far less commonly in temporary camp sites (one sure instance has been described). They hunted the great extinct animals, mammoth, mastodon, extinct species of bison and others, and their most characteristic artifacts were the large pressure-flaked stone points of the spears which they hurled from their spear-throwers, and the knives with which they cut the animals up. (A good instance is given by the Scottsbluff and Eden points and the Cody knife, which together make up the Cody complex.) Wormington quotes many different types of point, some of them from widely-scattered areas, and she confesses to being more of a 'splitter' than a 'lumper', but it may be questioned whether

¹ (1) *Ancient Man in North America*, by H. M. Wormington. 4th Ed. 1957. Denver Museum of Natural History, Popular Series, no. 4. (2) *Danger Cave*, by Jesse D. Jennings. *Memoirs of the Society for American Archaeology*, no. 14. *American Antiquity*, vol. XXIII, no. 2, part 2, October 1957.

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more than a few of the more special types are sufficiently standardized to indicate contemporaneity or a common authorship. If they do neither, the types cannot have much meaning, and the use of a common name can only be misleading.

With the help of radio-carbon dating, these hunting cultures have been correlated with the climatic oscillations at the end of the last ice age, the Wisconsin. Thus the last considerable advance of the ice (formerly called Mankato but now generally Valders) succeeded the Two-Creeks interstadial, which dates from about 10000 to 9000 B.C., and preceded the well-known Folsom culture, dating from about 8000 B.C. Similar hunting cultures, characterized by various types of large stone point and associated predominantly with the hunting of extinct species of bison, have been dated to between 7000 and 5000 B.C. Earlier than Folsom, and separated from it by a cold or wet period at least as old as the Valders advance, are remains left by men who hunted mammoth, horse and other animals with spears tipped by Sandia and Clovis points. The age of these remains is not known, but they can be assumed to be at least as old as the Two-Creeks interstadial and may well be considerably older. This presumption is strengthened by the presence of mammoth hunters in Mexico at an early date. The human skeleton found with mammoth at Tepexpan near Mexico City in deposits dated about 9000 B.C. geologically and by radio-carbon, is not certainly contemporary with the deposits but there is at least a probability that it is, and man certainly killed the same species at Santa Isabel Istápan near by, leaving stone points in its body.

This raises the question of how much earlier man can have arrived in the New World. It can confidently be assumed that he came from Asia across the Bering Straits at a period when so much water was locked up in the ice sheets that the Straits were dry, and at the same time Alaska was partly free from ice. Surprisingly, in view of its latitude, geological evidence shows that much of Alaska was never glaciated; such glaciation as occurred affected mainly the high ground to the south and the precise nature of its influence on human migration is difficult to assess. At any rate there were certainly several times during the Wisconsin glaciation when conditions suitable for the movement of arctic cultures could have been satisfied, and, in view of the wide distribution of the Clovis type of point, man's first arrival could have been well before Two-Creeks. A radio-carbon date of more than 20,000 years ago has been obtained from ivory in the Sandia Cave, but unfortunately it is not clear that this dates the Sandia points. Various finds in the West have been claimed indicate the presence of man far back in the Wisconsin glaciation, or even during the previous interglacial, and carbon dates of over 30,000 years have been obtained. At Tule Springs, Nevada, are split and burnt bones of camel, bison and horse with alleged fire pits, and on Santa Rosa Island, California, are skeletons of dwarf mammoths which appear to have been butchered, but further evidence is needed from both sites. At other sites in southern California are alleged stone implements and signs of fire, but most prehistorians accept neither the stones as human artifacts nor the fires as artificial.

At the other end of the scale, a way of life based more on the gathering of wild plant foods than on hunting has long been known in the Southwest, namely the Cochise Culture of Arizona and New Mexico, but since the first stage of it was dated by radio-carbon between about 5500 and 6000 B.C. it was assumed to be later than the hunting cultures of the Plains, with the corollary that it may have developed out of them. Work in recent years in the Great Basin, in Oregon, Nevada and Utah, has built up a body of evidence for a much greater age for this way of life, and this has been put on a firm basis by Jennings's work on Danger Cave. As a result of this, there is a series of carbon dates from two laboratories, which is not altogether free from inconsistencies but it gives a strong indication that the cave was occupied by food-gatherers as early as 9000 B.C.

NEW WORLD ORIGINS SEEN FROM THE OLD WORLD

The harsh environment of the Great Basin was exploited to the full by these people; they could not have survived if they had not done so, and the result was a very stable culture which continued with little change until the 19th century among tribes like the Northern Paiute. Jennings has named this the Desert Culture, and it forms the major item in what Wormington calls the Palaeowestern Tradition. Jennings regards the basket and the flat milling stone as the hallmarks of the Desert Culture, but the people did not live by gathering alone and there is plenty of evidence, in the shape of animal remains and feathers, as well as stone points for use with the spear-thrower, for the hunting of mountain sheep, antelopes and smaller game, including duck. The points are smaller than those associated with the big-game hunters of the Plains, and they include stemmed and notched types which were thought until recently to be confined to much later times. Stone points, blades and scrapers are in fact the most abundant artifacts in Danger Cave, and Jennings believes that this is because they were easily made, easily blunted, and hence discarded, unlike other things, before they appeared to be worn out. In a largely vain effort to establish a chronological sequence, he splits his stone tools into numerous types, some of which rest on as few as two examples. This provokes a warning to the 'splitters'; we suggest that they might take some lumps of chert and obsidian and see how many types they produce by accident and how many variations arise fortuitously as the result of a chance or lucky break.

The deposits in Danger Cave are divided into five main levels, but it is not implied that the occupation was continuous even within one level. There is every reason to believe that the people had to travel to other places each year in order to find their food, in the same way as their 19th-century successors. Apart from relatively unimportant variations in stone tools and weapon forms, it is suggested that the Desert Culture was essentially uniform over the Great Basin from at least 7000 B.C. to about 3000 B.C., after which variations both in time and place were produced by the accretion of new traits. The bow, for instance, seems to have been introduced, with new types of very small point, shortly before the beginning of the Christian era. An exceedingly important fact, which supports data previously obtained from Fort Rock Cave, Oregon, is that twined basketry, with specialized variations such as three-strand twining and (at Fort Rock) false embroidery, was in use by 7000 B.C.

The early dates obtained for Danger Cave raise certain difficulties. This and other caves lie in a basin which was submerged by lakes which filled it to levels above the caves during pluvial periods, but it is perfectly clear that they have always remained dry since their first occupation by man. Geologists have correlated the pluvials which gave rise to the lakes with glacial advances, and one of these, which would have submerged Danger Cave, has been ascribed to the Valdres alias Mankato advance, succeeding the Two-Creeks interstadial, which is dated at about 9000 B.C. The implication is that either the carbon dates for Danger Cave, or those for Two-Creeks, or both, or the geologists' correlations, are wrong. The Danger Cave samples have always been dry, and it is difficult to imagine any possible source of error such as sometimes arises from samples taken under wet conditions. Again, the late glacial sequence of which Two-Creeks is a part has been checked over and over again and correlates very satisfactorily with the European sequence, which is dated quite independently, not only by radio-carbon but also by varves and pollen analysis; Two-Creeks corresponds to the Allerød Stage in Europe. Jennings assumes, we think with reason, that both sets of dates are right and that some geologists are wrong in assuming that a glacial advance in the Eastern United States would necessarily produce a pluvial in the Great Basin, or at any rate that the Valdres in particular would produce a result on the scale suggested. He suggests that Antevs, the geologist who has been most active in these correlations, has in fact tacitly abandoned this position since he now ascribes the latest pluvial

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in the Great Basin to an earlier period than formerly. Another fact which must be faced if these early dates are accepted is the absence from Danger Cave of such extinct animals as ground sloth, horse and camel, which were hunted by man of comparable date elsewhere in the West. For the present it can only be assumed that they were already rare or absent in the Danger Cave area, just as some forms like mastodon were absent from the West at this time. The main result of all this work is, then, that the Desert Culture of the West is as old as the better-known big-game hunting cultures of the Plains, Folsom and its contemporaries and successors, and that it did not develop from them. It can not yet be claimed that there were no big-game hunters in the New World before the gatherers, but it is at least possible that the differentiation between the two ways of life took place before the entry of either into the New World. Whether this is so or not, a consideration of the evidence about specific traits at Danger Cave and other Western sites leads Jennings to some speculations which may provoke lively reactions. If the dates are right, twined basketry and perhaps other weaving techniques were widely practised in Western North America earlier than 7000 B.C., before there is any known record of them in the Old World. It may be added that the degree of specialization shown indicates that twining was very considerably older than this date. It is therefore possible that these are American inventions which were introduced into Eurasia! To this we may add that the technique of oblique parallel flaking applied to projectile heads, which is found in the early American hunting cultures, is unknown at comparable dates in Eurasia, although it is well represented at pottery-bearing sites in the Lake Baikal region and on the Middle Lena.² Jennings believes that the same may apply to the collection and milling of small grains, and consequently perhaps gruel cookery, which were known over the desert West and Middle West by 8000 B.C. It may be that Eurasia cannot produce these processes at such an early date—even at Jericho!

In a word, it would appear that the rapidly developing picture of the earlier American cultures is already pregnant with conclusions affecting the world pattern of culture growth and diffusion from late Pleistocene times onwards. To discuss, even in outline, all the issues raised would obviously be beyond the scope of this article; it is proposed merely to deal in detail with one important question as it strikes the observer from the Old World, namely, the nature and origin of the first New World settlement. In the last number of *ANTIQUITY*, 1959, 44, an interpretation of this problem was offered by C. S. Chard, based specifically on North-Eastern Siberian data; an alternative reading, outlined below, is put forward by the writers in the belief that it accords better with the overall picture of Old World prehistory suggested by a number of important recent discoveries.

Several of these discoveries are indeed so new and so unexpected that they can hardly be said as yet to have been fully digested; still less have their implications for local sequences been adequately considered. This is especially true of regions like the Soviet Union, where for one reason or another investigators often appear to be out of touch with current research in the West. None the less it seems to us that certain conclusions already emerge with sufficient clarity to justify the formulation of a working scheme into which the American evidence may reasonably be incorporated.

One aspect of the Palaeo-Indian cultural scene, above all, is bound to strike the Old-World prehistorian as contrasting strongly with his own, namely, the predominance in virtually all lithic assemblages, throughout both continents and over a period of at least 15,000 years, of a single specialized tool class—bifacially worked projectile heads. The

² See for example Michael, H. N., 'The Neolithic Age in Eastern Siberia', *Trans. Amer. Phil. Soc.*, New Series, vol. 48, part 2, 1958, figs. 33, 87. Since writing this article we have seen that Paul Tolstoy had already drawn attention to this point. See 'The Archaeology of the Lena Basin and its New World Relationships, Part II', in *American Antiquity*, XLIV, no. 1, July 1958, p. 65 and fig. 1.

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fundamental nature of the difference may be realized when it is recalled that this highly distinctive culture trait is totally unknown in South-West Asia for instance before Neolithic times. In Western Europe it occurs only during one short episode in the course of an unbroken cultural record of 25,000 years of Upper Palaeolithic development. In Eastern Europe its occurrence is less isolated, but there also it is only numerically an important element for a relatively short time, subsequently surviving as a rare (if fully evolved) device, presumably intended for some restricted purpose or function.

The Old World sequence differs, of course, in many other and more fundamental ways from that of the New World, above all in the relative immensity of its duration. Recently published potassium-argon dates fully confirm previously held notions of an age of some 400,000 years for the beginning of the great hand-axe tradition. This, the first fully recognizable and standardized pattern of human activity, can certainly be detected from India to the Mediterranean, throughout Africa, and in South-West Europe. Outside this area, notably in Central and Eastern Europe and Northern Asia, its presence is still problematical, while in South-East Asia Movius' proposed Chopper-Chipping-Tool complex seems to offer evidence of a distinct and approximately contemporary tradition.

Throughout its vast area of distribution the hand-axe complex of the Lower Palaeolithic underwent distinctive and astonishingly uniform changes leading to the Middle Palaeolithic. This event seems to have taken place generally towards the end of the Middle Pleistocene, say, approximately contemporary with the Penultimate Glacial maximum of the European succession. The cultural changes in question constitute little short of a complete revolution in tool design and methods of manufacture. The most striking features are a marked reduction in average size of finished tools, coupled with much greater reliance on sharp cutting edges at the expense of heavy more or less pointed forms. There can be little doubt that the change reflects a profound evolution of function, very probably connected with greater development of tools-for-making-tools. The Middle Palaeolithic innovations are obviously correlated with methods of striking the flake blanks from which the finished implements were fashioned; the same ingenious primary flaking methods can be recognized from India to the Atlantic and from Southern Britain to South Africa.

Superimposed on this basic uniformity a certain degree of regional specialization eventually becomes apparent, and some of the variants so produced seem to have penetrated areas not certainly occupied before by man. This is true, for instance, of Eastern Europe, where a distinctive Middle Palaeolithic tradition can be recognized from Tashkent, on the western fringe of the Himalayan massif, to the Alps.³

During the past five years notions regarding the human strains responsible for these cultural events have been considerably modified. With the elimination of Piltdown and the discovery of *Atlanthropus* the whole case for a precocious appearance of *Homo sapiens* of modern type during the Lower Palaeolithic cycle has, in the opinion of many specialists, virtually disappeared.⁴ On the contrary we now have positive proof of association between a human strain comparable in anatomical status to *Sinanthropus* and an early variant of the hand-axe tradition.

As for the human types associated with the Middle Palaeolithic, it is still apparent that these differed significantly from modern *Homo sapiens* (with the possible exception of Fontéchevade,⁵) although they do so to a very varying extent. On the whole, the populations of Western Europe seem to be the most divergent, and those of Western Asia the least

³ We gather that current discoveries about to be published will extend this distribution appreciably further to the North-East.

⁴ See for instance Piveteau, *Traité de Palaeontologie*, vol. 7, Paris, 1957.

⁵ Vallois, *Archives de l'Institut de paléontologie humaine*, 29. Mémoire, Paris, 1958.

divergent, from modern man. This hint of a return to the immemorial theory of a Middle-Eastern Garden of Eden suggested by the fossil evidence is strongly reinforced by the discovery of a possible prototype Upper Palaeolithic in the same area and its geological dating to the end of the last Interglacial.⁶ It is further enhanced by certain notions concerning the nature of the Middle to Upper Palaeolithic transition. Industrially the difference may be compared to the Lower to Middle Palaeolithic change in that a profound alteration in the range of tool forms is again closely correlated to the primary technique of striking the flake blanks. In the Upper Palaeolithic this involves the introduction of narrow, parallel-sided struck-blades in place of the wide polygonal-sided flakes of the Middle Palaeolithic. Many Western prehistorians are now convinced that this technological change, unlike the earlier transition from Lower to Middle Palaeolithic, was no mere process of widespread convergent development. On the contrary, it seems to represent the rapid dissemination of a sharply defined technique, that of flaking with a carefully designed intermediate tool or punch. The significance of this theory lies in the strong implication of diffusion of the whole culture complex from a single original centre, most probably as the result of widespread migration. A definite migration of this sort has long been favoured by human palaeontologists, and the results of modern radio-carbon dating now bring further support by suggesting that the cultural tradition had spread across the whole area from Eastern Afghanistan westwards to the Atlantic within two or three thousand years. At least it now appears certain that the substitution was virtually complete by 30000 B.C. Nor can there be any question that the populations practising this industry were not exclusively of modern anatomical status. Here and there a few pockets of the presapient 'Neanderthaloid' makers of the Middle Palaeolithic may have survived for a short time, but with the probable exception of the Atlas massif in North Africa the change must have been complete by this date.

In its essence this conception of the Old-World sequence has had a long history; a new element is, however, supplied by recent finds and re-appraisals of the so-called Solutrean and Proto-Solutrean complex characterized by the first pressure-flaked projectile heads in Europe. In South-West Europe this element makes its first appearance subsequent to the two major initial phases of the Upper Palaeolithic—the Aurignacian and Gravettian. In Eastern and Central Europe on the other hand, from the Don basin to Southern Germany, it is a characteristic of precisely the first phase. It has now been conclusively shown, moreover, that this type of tool was an invention of the preceding Eastern Mousterian phase of the Middle Palaeolithic.⁷ It is true that many of the earliest stages of the Upper Palaeolithic in Eastern Europe, notably in South Russia, are of relatively recent discovery and that it cannot yet be claimed that they supply a detailed sequence such as we possess for Western Europe. The first tradition to emerge in full detail from the more fragmentary archaeological record here is the great East Gravettian culture, whose importance for the whole Upper Palaeolithic of Europe daily becomes more apparent.

This remarkably developed cultural manifestation indeed preserves its full identity in art, industry and implied religious practices from the Valley of the Don for 1300 miles westwards to Moravia, and highly probable signs of its influence can be recognized throughout Western Europe to the Atlantic, a total distance of over 2000 miles. Little is yet known of its eastward extension, since Kazakhstan and the South Central Siberian plain are as yet undocumented during this period. As far as our present information goes it would, however, appear that the real centre of the culture may well have been in fact in Southern

⁶ Garrod, D. A. E., *Proc. Int. Congr. Prehist. and Protohist. Sci.* Hamburg, 1958. (In press.)

⁷ Zotz, L. F., *Das Palaeolithikum in den Weinberghöhlen bei Mauern*. Bonn, 1955. McBurney, C. B. M., 'The Geographical Study of the Older Palaeolithic Stages in Europe, P.P.S., 1950; *Proc. Int. Congr. Prehist. and Protohist. Sci.* Hamburg, 1958. (In press.)

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European Russia. If so, there would be nothing surprising in the apparent indications of its influence in the most easterly known province of the Old World Palaeolithic from Lake Baikal to the Verkholsk Mountains. This region, whose western limits lie almost exactly half-way between the Don and the Bering Straits, as Chard has pointed out, is a crucial one in any discussion of possible Palaeo-Indian origins. The first traces of human occupation so far recognized occur at Maltà and Buret, two sites just west of Lake Baikal. They comprise camp sites of mammoth and reindeer hunters practising a true blade industry, but one which displays peculiarities unknown further west. At the same time, it lacks many of the distinctive features which characterize the East Gravettian in the Don basin. In some respects indeed the forms have a distinctly archaic flavour reminiscent of the very early phases of the Upper Palaeolithic in the West. In the stonework almost the only specifically East Gravettian trait is the presence of rather unusual fluted artifacts resembling coarse polyhedric burins. Apart from this, the suggestion of affinity with the former culture comes almost entirely from the carved and decorated bone objects. Most important is the presence of female figurines recalling in many respects the most characteristic single motif of East Gravettian art. Since this design is certainly shared by contemporary cultures as far to the West as the Pyrenees there would, on the face of it, be nothing strange in an equally extended distribution to the East. In addition, one may note the presence of eyed needles and naturalistic animal figures and engravings. A second type of industry practised by the mammoth and reindeer hunters of the same general region has long been regarded as distinct from the above and is assumed to be later. The evidence regarding the relative chronological positions of these two is not perhaps above criticism, though both are unquestionably of full Pleistocene age. From the details of the well-published site of Afontova,⁸ near Krasnoyarsk, it is possible to study the difference between the two which appear to us to be not quite as clearly marked as some writers maintain. Basic features in common are several specialized scraper forms and eyed bone needles, which both presumably indicate evolved fur or leather working. The polyhedric burins are also present.

The principal difference between the two Siberian assemblages, however, resides in the presence at Afontova, together with the evolved Upper Palaeolithic forms just mentioned, of a greatly enhanced proportion of archaic Mousterian-like elements. Not only do these include typical Mousteriform side-scrapers with step flaking and other Middle Palaeolithic details of technique, but there are in addition a number of forms such as plano-convex points and miniature scraper-points which, as it happens, are distinctive of the Eastern Mousterian. The latter culture has not yet been certainly demonstrated east of Tashkent,⁹ yet the coincidence of forms is certainly remarkable and one cannot help wondering if the possibility of an earlier culture stratum of East Mousterian character in Central Siberia has not been too easily dismissed. The curious 'hybrid' culture of Afontova seems indeed to have preserved its identity far into post-Glacial times, and it would appear that this survival has encouraged a belief in a correspondingly late date for the beginning of the complex. We are inclined to think that this late dating so widely accepted requires more specific support than has so far been produced.

A third group of sites from the region of Kiakhta South-East of Baikal combine the features noted above with the interesting addition of true bifacially worked projectile heads. They also are assigned by A. Abramov and others to the Palaeolithic cycle.¹⁰

The absolute date of these central Siberian finds is obviously of first importance when

⁸ Auerbach, M. K., *The Palaeolithic Stage of Afontova Gora III*. Irkutsk, 1930.

⁹ We understand that certain new finds about to be published may extend this distribution appreciably further East.

¹⁰ Abramov, A., *Materiali y Issledovania p.o. archeologia S.S.S.R.*, vol. 45, 1953.

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attempting to assess their possible relevance to American origins. Following A. P. Okladnikov, American investigators seem to be satisfied that none are earlier than the very end of the last Glaciation. Yet in the case of Maltà and Buret it is arguable that the East Gravettian elements they contain belong to the early phase of that tradition, and this has recently been carbon-dated in Moravia to 24000 B.C. Female figurines and naturalistic animal figures are atypical or virtually absent from the apparently later variants of the tradition found at Mezine, Borshevo, Kostienki IV. Again it may be noted in passing that the very late date assigned to Kostienki IV by Rogachev¹¹ appears to be based in part on outdated notions of the eastward extension of the French Magdalenian and Solutrian (both purely local cultures) and in part on non-archaeological arguments deriving from dialectical materialism. In fact, there seems to be no good reason why the culture represented at Buret and Maltà should not date from, say, 20000 B.C. in round figures (allowing for the time lag involved by the diffusion), or slightly before.

Further observations of interest in this connection may be quoted from two famous sites south of the Ordos plateau, Shue Tong Keou and Sjara Ossa Gol, some 1000 miles south of Lake Baikal across the Gobi Desert.¹² These demonstrate the penetration to that remote area of an industrial tradition whose true blade and burin elements are readily apparent despite the limitations imposed by the peculiar local raw material, consisting of small non-flinty pebbles. The presence of such culture traits in the heart of the supposed Chopper-Chopping-Tool area of survival in a well-dated Pleistocene deposit is a fact of great significance. A further discovery of almost equal importance in this connection is that recently announced of a flake industry from the middle reaches of the Fen River in the same general region. This industry, in which only the eye of faith can distinguish the slightest traces of Chopper-Chopping-Tool influence, is undeniably of general Middle Palaeolithic character in the Western sense. It can be dated geologically to a slightly earlier period than the two Ordos sites and is associated with what is apparently a non-sapient human strain.¹³

In a word, the whole trend of current discovery in the Far East and contiguous regions, far from providing support, may well suggest on the contrary that the whole Chopper-Chopping-Tool hypothesis is in need of careful overhaul. However convincing the conception may appear of an extremely elementary and functional tool pattern of this sort at the end of the Lower or beginning of the Middle Pleistocene, its survival as an active tradition for a quarter to a third of a million years is quite another matter. Again, it may be regarded as strange that such an astonishingly conservative and impoverished tradition should suddenly give rise to the highly specialized notion of pressure-flaked projectile heads. In Central and Eastern Europe over a very wide area these weapons first appear in evolved form as a characteristic product of culture fusion of precisely the elements that we suspect in Central Siberia, namely East Mousterian and Early Upper Palaeolithic. It is true that this trait did not, as far as we can tell, become current in the earliest Upper Palaeolithic there. Central Europe warns us, however, that culture fusion of the type we have in mind is a complex process; at the Cave of Istallöskö in Hungary, for instance, the lower layer is purely Middle Aurignacian, and the Szelethian influence with its bifacial technique only becomes apparent in the second layer. The study of acculturation shows that it is by no means uncommon for an intrusive culture to preserve its identity intact for an appreciable time before fusing with indigenous elements. The picture offered by the lower layers of

¹¹ Rogachev, A. N., *Ibid.*, vol. 39, 1953.

¹² Boule, M., and others, 'Le paléolithique de la Chine', *Archives de l'Institut de Paléontologie Humaine*, vol. 4, 1928.

¹³ Movius, H., 'New Palaeolithic Sites near Ting-Ts'un in the Fen River, Shansi Province, North China', *Quaternaria*, vol. 111, p. 13 ff., 1956.

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Kostienki I is in this respect analogous. The projectile heads of the lowest layer are lacking from the small settlements immediately succeeding it, but reappear later at many stations of the classic East Gravettian.¹⁴

Time will show whether we or Dr Chard have the better case, and an important part in future discussions is no doubt destined to be played by C.14 readings which we understand are shortly to be undertaken. But however these turn out we feel that much remains to be filled in in the picture of the supposed later Chopper-Chopping-Tool cultures. Above all, their geographical limits will need to be more systematically defined, since it is already clear that important areas formerly attributed to them were in fact the scene of Middle and Upper Palaeolithic episodes far more nearly analogous to those of the West than used to be supposed. In short, in seeking the ultimate origin of the Palaeo-Indian hunting cultures we are still inclined to attribute a dominant role to the great events of Upper Palaeolithic spread between the 25th and 20th millennia B.C. rather than to possible survivals of primitive indigenous traditions of the Far East.

In conclusion, it may be useful to emphasize the possible significance of an element in the American evidence itself. The first concerns the character of the overwhelming majority of the known older hunting sites. In contrast to most of the Old World finds they appear to represent 'butchering' sites rather than true camp sites¹⁵; that is to say, they are open stations where wounded game happened to die and the information is largely confined to fragments of implements of the chase, with little or no record of the traces of everyday existence that survive from a true camp. Lindenmeier is a notable exception, but it is not by itself sufficient to redress the balance. The possible significance of the distinction between the two types of site is suggested by an isolated find at the Mexican site of Santa Isabel Istápan mentioned above. Here in conditions of unimpeachable association with a mammoth kill and projectile points (of rather unspecialized finish) were two fragments of struck blades that suggest very strongly indeed the punch technique of the Upper Palaeolithic. If this were really evidence of diffusion of this crucial trait from Central Siberia to Mexico, then other sites ought of course to show it in the intervening area. So far none of the butchering sites which have largely monopolized attention with their spectacular fauna and projectile heads, have done so. But what is this negative evidence worth? We should like to suggest that it can only be fully relied on after the careful study and publication of a representative series of workshop sites and seasonal and semi-permanent camp sites. In seeking to establish the full range of auxiliary tools and basic processes of production it will be necessary to classify and record adequate samples of cores, waste flakes, unprocessed blanks, resharpening flakes, unfinished specimens of all kinds and of course all awls, scrapers, utilized flakes and the like, however variable and widely distributed these may appear at first sight. In short, now that the association between certain projectile heads and various extinct species of quarry has been abundantly established, we would like to address a plea to the scholarly 'splitters' that they shift their gaze from the minute variations of a single-tool type to take a wider view of their material.

In the Old World we have learned painfully how misleading the study of isolated traits can be when the real objective is to reconstruct the evolution and interaction of cultures in their biological and geographical environment. Pure typology may be suitable for the archaeology of metal and ceramic types for instance, but with lithic industries expressing a much greater range of activities it can never be more than one element in a far more comprehensive and inductive approach.

¹⁴ Evidence summarized in Childe, V. G., *12th Annual Report of the University of London Institute of Archaeology*, p. 8, 1956.

¹⁵ We are indebted to Dr Wormington for making this point to us in conversation.

The Kingdom of Peleus and Achilles

by R. HOPE SIMPSON and J. F. LAZENBY

The authors, who are both working at the British School at Athens, here discuss the identification of a number of Homeric sites on the basis of archaeological evidence.

‘AND now I will tell of the men who lived in *Pelasgian Argos*, who dwelt in *Alos* and *Alope*, in *Trechis*, *Phthia*, and *Hellas* of the Fair Women. Their names were Myrmidons and Hellenes and Achaeans; and they and their fifty ships were under Achilles’ command.’ (*Iliad*, II, 681–5.)

That the Kingdom of Peleus was centred on the Spercheios Valley is sufficiently indicated by the dedication of his son Achilles’ hair to the river (*Iliad*, XXIII, 140 ff.), and by the genealogy of Menesthios (*Iliad*, XVI, 173 ff.). The valley and the coastal plains on the shores of the Malian Gulf are separated by mountain ranges from the districts to north and south, and would naturally form a political unit, while there is no suggestion in Homer that Peleus’ power extended outside this area. It was such an undistinguished part of Greece in historical times that it is unlikely that a tradition locating the greatest hero of the Trojan War there can have been fabricated at a late date.¹

Previously few prehistoric sites were known in the region of the Spercheios Valley,² and the only Mycenaean finds were the contents of a L.H. III cist grave near Vardhates.³ In April we set out to discover the full extent of the prehistoric inhabitation of the district, and we list here (from east to west) the positive results of our researches.⁴ (See map.)

The citadel of *Larisa Cremaste*⁵ lies to the north of Pelasyia village (in Leake’s day Gardhiki). The site commands the entrance to the Malian Gulf and the coast route from Lamia to Volos. Besides pottery ranging from classical to Byzantine, we found here two Mycenaean (L.H. III) sherds.

On the acropolis of *Echinous*⁶ (PLATE XIV (a)), near the modern Akhinos, among classical and Hellenistic sherds, were a few Mycenaean (L.H. III B) and a piece of Middle Helladic Grey Minyan ware, as well as implements of brown chert. The chamber tombs near the hill are almost certainly Mycenaean.⁷ The site, like Larisa Cremaste, has a fine strategic position, and the plain could have supported a considerable population.

At Platania, 4 km. to east of Lamia, about 100 m. south of the main Lamia–Volos road

¹ Whereas his location, e.g. at Pharsala (*Schol. Odyssey*, IV, 9), is easily explained in this way. For the best previous discussion of the Kingdom of Peleus cf. Allen, *The Homeric Catalogue of Ships* (1921), 108–113.

² The main site was Lianokladhi (Wace and Thompson, *Prehistoric Thessaly*, 171–92). Surface finds were made at Amouri (*op. cit.*, 11).

³ Marinatos, *Bericht über den Intern. Kongress für Arch.* (1939), 334.

⁴ In *B.C.H.* 70 (1946), 636 (cf. *Prehistoric Thessaly*, 5 and 240). Professor Wace indicated the gaps in our knowledge of the Spercheios Valley and Malis. Our work is largely the result of his personal encouragement.

⁵ Paulys, R. E., XII¹, 840–5; Béquignon, *La Vallée du Spercheios* (1937), 140–1. The walls are of Irregular Trapezoidal masonry, and probably 5th century B.C. (Scranton, *Greek Walls* (1941), 71–3 and 169).

⁶ Béquignon, *op. cit.*, 299–303, fig. 12, and pl. xv.

⁷ *A.J.A.*, 46 (1942), 500, fig. 2.

THE KINGDOM OF PELEUS AND ACHILLES



The Spercheios Valley and the Malian Gulf. 1. *LARISA CREMASTE*. 2. *ECHINOUS*. 3. *PLATANIA*. 4. *TRACHIS*. 5. *RAKHITA*.

is a farm (PLATE XIV (b)) standing on the site of a large prehistoric settlement, which measures about 170 m. by 150 m., and is 7 m. to 8 m. high. To the north, below the road, a perennial spring empties into a marshy pool surrounded by rushes. The sea, now 3 km. to the south-east,⁸ must have once reached nearly up to the site. Surface sherds included several Neolithic (most classes are represented, including the earliest), a few Early Helladic, and an amount of Middle Helladic and Mycenaean (L.H. III B-C) which seems surprising for a low mound site. There was also a geometric sherd. The position of the settlement is reminiscent of Lerna, and its size is as great as that of Lianokladhi.

The castle of Zituni, the ancient citadel of *Lamia*,⁹ completely dominates the head of the Gulf, the lower part of the Spercheios Valley, and the Phourka pass north into the plain of Pharsala. The early walls of Lesbian style on the acropolis¹⁰ are probably of the 6th century B.C., and we found a geometric sherd near the summit. Because of the medieval and later structures on the acropolis it is difficult to test whether the hill was occupied in prehistoric times.

On the lowest terraces of the site of classical *Trachis*¹¹ we recovered, with difficulty, three Mycenaean sherds. It was here rather than on the bare upper slopes of the Phrourion marked by Béquignon that the Mycenaean settlement was situated. Protection would have been afforded by the steep Trachinian cliffs above, and water by the Asopus and nearby streams, while, as Herodotus says (VII, 199), this district contains the largest area of farming land between the mountains and the sea.

About 1½ km. to the north-west, on similar terraces at the foot of the cliffs (PLATE XV (a)),

⁸ The western shore of the Malian Gulf once lay a considerable distance inland of the present coastline (cf. Pausanias, x, 20, 7).

⁹ Béquignon, *op. cit.*, 263-78, figs. 5-6, pl. XII.

¹⁰ Scranton, *op. cit.*, 160. Béquignon, *op. cit.*, pl. XII, 2.

¹¹ Béquignon, *op. cit.*, 243-60, fig. 4, pls. IX-X.

is a prehistoric settlement of moderate size, which produced Middle Helladic and Late Mycenaean pottery (L.H. III A-B). The district is named Rakhita. To the north-west lies the village of Vardhates, near which the Late Mycenaean cist grave was found.

Further up the valley we explored the classical hill sites of Hypati,¹² Kastrorakhi¹³ (FIG. 1, the westernmost cross on the inset), and Kastri¹⁴ (PLATE XV (b)). Although we found no prehistoric sherds on any of these sites, we suspect that Kastri was occupied in Mycenaean times. If so, it might mark the limit of the prehistoric occupation of the Valley.¹⁵

The discovery of Mycenaean settlements at Trachis and Rakhita nearby has put beyond reasonable doubt the identification of one of the towns in Peleus' Kingdom and has created a fair presumption that this entry in the *Catalogue* is based on a genuine Mycenaean tradition.

Achilles repeatedly refers to his home as *Phthia* (e.g. *Iliad*, I, 155; IX, 363-4), occasionally as *Phthia* and *Hellas* (e.g. *Iliad*, IX, 395; *Odyssey*, XI, 496), and these are usually taken as districts rather than towns, although this was already disputed in Strabo's day (IX, 5, 6). If they were towns it is difficult to account for the presence of Phthians under Protesilaus and Philoctetes (*Iliad*, XIII, 685 ff.), or Panhellenes under Aias the Less (*Iliad*, II, 530¹⁶), whereas if they were districts we may suppose that Phthians once occupied a wider area than the Spercheios Valley, and were pushed out of the latter by the invading Myrmidons and Achaeans, leaving the name behind them. Achilles' men are never referred to as Phthians. Hellenes managed to survive in both Peleus' and Aias the Less' domains, which suggests that the district *Hellas* lay in the Valley proper and perhaps particularly to south of the river.¹⁷ As was noted by Leake,¹⁸ and later by Wace and Thompson,¹⁹ the Spercheios was until very recently called 'Hellada'. Of the other proper names in the *Catalogue* passage, *Pelasgian Argos* is taken by Allen²⁰ to refer to the Maliac plain. H. M. Chadwick, however, suggested²¹ that it might refer to what was later called Larisa Cremaste (where we found some evidence of Mycenaean occupation. The name of the citadel of Argos (*Pausanias*, II, 24, 1) shows that there was some connection between the names Larisa and Argos, and Strabo (IX, 5, 13) says that an alternative name for Larisa Cremaste was Pelasgia. It is true that the hills west and south of Larisa Cremaste cut it off to some extent from the coastal strip and give point to Strabo's statement that it lay in Protesilaus' rather than Peleus' territory, but beyond it to the north the country becomes wilder still. The balance of the *Catalogue* passage is enhanced if the first four names refer to towns, and *Phthia* and *Hellas* to districts—the one to the coastal area east of Lamia, the other to the lower valley of the Spercheios proper. Elsewhere however in the *Catalogue* (*Iliad*, II, 536, 581, 603) the district is mentioned first, and 'Achaean Argos' normally seems to mean the Peloponnese generally (*Iliad*, IX, 141 and 283; *Odyssey*, III, 251): only once (*Iliad*, XIX, 115) does it seem to be used in a manner comparable with the proposed meaning of Pelasgian Argos.

Trechis must refer particularly to the site of classical Trachis, but also probably to the

¹² Béquignon, *op. cit.*, 308-12, fig. 13, pl. xvi, 1.

¹³ Cf. Béquignon, *op. cit.*, 313-15, fig. 14, pl. xvi, 2.

¹⁴ The site (previously unrecorded) is on the north bank of the river.

¹⁵ Professor Wace thought that the upper half of the valley had been part of a primeval forest belt (*Prehistoric Thessaly*, 240, cf. 6-7). To west of Kastri the valley is now lightly wooded.

¹⁶ Cf. Allen, *The Homeric Catalogue of Ships* (1921), 54.

¹⁷ *Op. cit.*, 113.

¹⁸ *Travels in Northern Greece* (1835), II, 8.

¹⁹ *Prehistoric Thessaly*, 255.

²⁰ *Op. cit.*, 108 ff.

²¹ *The Heroic Age* (1912), 280, n. 1.

THE KINGDOM OF PELEUS AND ACHILLES

district, including the settlement at Rakhita. *Trechis* would have been the frontier town of the Kingdom in this direction, since the Pass of Thermopylae is the natural boundary, and the sulphur springs between Trachis and the Pass make this area uninhabitable.

Alos is the most obscure, and was already so by the time of Strabo (ix, 5, 8). The name would fit a town on the fringes of the marshy lower valley, such as Platania. Nevertheless it is barely credible that the hill of Lamia, which has the finest strategic position in the Valley, would have been overlooked by the Mycenaeans.

Alope is hardly less obscure, but here at least we have a definite statement by Stephanus of Byzantium (ad v.) that there was such a place between Larisa Cremaste and Echinous.²² The only sites where we found Mycenaean sherds on the north coast of the Malian Gulf were Larisa Cremaste and Echinous themselves.²³ If Larisa Cremaste is *Pelagian Argos*, we are left with Echinous, and it seems likely that Stephanus or his sources were induced by the existence of the classical town to put *Alope* elsewhere.

The placing of *Alos*, *Alope*, and *Trechis* respectively at or near Lamia, at Echinous, and at Trachis, need not imply that the Mycenaeans occupied only the lower valley and the shores of the Malian Gulf. Hammond²⁴ suggested that the Agrinion-Karpenision pass was used by migrating prehistoric tribes, and Phoenix' position, ruling the Dolopes on the frontiers of Peleus' kingdom (*Iliad*, ix, 484) may be part of a genuine tradition showing that the Mycenaeans were aware of dangers from this area, for Thucydides (ii, 102, 2, cf. *Polybius*, xxi, 25) shows that Dolopia was located west of Tymphrestos later.

Of the towns in the Kingdom only one—*Trechis*—has been positively identified. Nevertheless it has been proved, by finds at Trachis, Rakhita, Platania, Echinous, and Larisa Cremaste, that Mycenaean culture did penetrate to this region; and the picture we suggest, with the four main towns near the head of the Gulf, makes sense from a strategic point of view. Anyone who holds these four towns holds the valley with all the important communications which lie through it. The district was apparently as fertile in Homeric times²⁵ as it is today, and a potential strength of fifty ships (and 2500 men?—*Iliad*, xvi, 168–70) seems not impossible.

²² Livy (xlii, 56) refers to the attack by the Roman fleet under Q. Marcius Rex on Larisa Cremaste and Alope in 171 B.C., which suggests that Alope was near Larisa Cremaste. However, since the fleet eventually sailed to Chalcis, it must have passed Locrian Alope, and it may have been this town which was taken by it.

²³ The remains at Rakhes (Béquignon, *op. cit.*, 367–8, pl. xx, 2) are insufficient to support the conjecture that Alope was here.

²⁴ *B.S.A.*, 32 (1931–2), 149.

²⁵ *Hellas* is described as 'with broad dancing places' (*Iliad*, ix, 478) and *Phthia* as 'rich in loam', 'feeder of men', and 'mother of sheep' (*Iliad*, i, 155; ix, 363 and 479).

Mottes : A Classification

by D. F. RENN

Mr Renn is by profession an actuary, and is interested among other things in the application of mathematical techniques to archaeological problems. This article is the outcome of ten years spare-time study and field survey of sites in England and Wales.

IN 1878, General Pitt Rivers excavated the motte and bailey castle on the downs behind Folkestone and published the results with characteristic promptness and accuracy.¹ Today, over three-quarters of a century later, only four excavations, all in the past ten years, have advanced our knowledge further.² While these have solved many problems, they have posed others, for few mottes are adequately documented (Old Aberystwyth is an honourable exception) and the dating of results can only be approximate.

There are an infinite number of ways of laying out a motte and bailey, even where the site has natural limitations, and closely similar plans show a contemporary solution to the ageless problem of attack and defence. Most mottes are circular, but we may distinguish:

- I. The oval (diameters differing by 20 per cent or more) and
- II. The angular (sides at definite angles to each other).

In elevation we have:

- A. High (height greater than minimum top diameter).
- B. Low (pudding-shaped).
- C. Crater (pronounced ringbank).

The bailey may be:

- a*, circular; *b*, oval; *c*, triangular; *d*, quadrilateral;
- e*, lobed; *f*, polygonal.

and the relation of motte to bailey may be:

1. Central (own separate ditch). 2. Internal (inside bailey ditch).
3. Peripheral (astride bailey ditch). 4. External (outside bailey ditch).

Any classification should result in a homogeneous group with similar major features; the test of this rather mechanical grouping is whether or no the groups are consistent in date or purpose. Group Be2 (which includes the writer's local motte) consists of Pencader, Carmarthenshire (1145, *Brut y Tywysogion*), Tomen y Rhodwydd, Denbighshire (1149, *ibid.*), Kilpeck, Herefordshire (remodelled after 1139?), South Mymms, Middlesex (1141-2, Round, *Geoffrey de Mandeville*) and Bryn Llwyd, Radnorshire. Each site lies beside a ridge-way, on the sunny slope near the head of a valley; there is thus close similarity of date, design and siting (FIG. 1).³

A group of unfinished castles (IIBd2) lie in an arc north of Cambridge, about ten miles apart. They are most probably those begun by Stephen in 1143-4 to check the depredations of the former Earl of Essex, Geoffrey de Mandeville. Burwell⁴ and Lidgate covered the Icknield Way, Caxton the Ermine Street and Rampton the Fen causeway to the Isle of

¹ *Archaeologia*, XLVII, pp. 429-65.

² Mr B. K. Hope-Taylor at Abinger (Surrey) and Mote of Urr (Kirkcudbright); Mr D. M. Waterman at Clough (Co. Down) and Mr C. H. Houlder at Old Aberystwyth (Cards.).

³ A site of type Bd3, recently discovered by aerial photography, is circumstantially dated to 1115-30.

⁴ Excavated by Mr T. C. Lethbridge (*Proc. Camb. A.S.*, xxxvi, pp. 121-33).

MOTTES: A CLASSIFICATION



FIG. 1.

Ely.⁵ Further north, the castle of Ramsey was probably due to Geoffrey himself. Stephen, however, failed to smoke out the hornet's nest of adulterine castles near Geoffrey's erstwhile keep at Walden, and only the latter's death in an attack on one of the blockading castles (probably Burwell) led to the collapse of organized revolt in the region (FIG. 2).

So two tests give promise of useful results from the classification. But the majority of mottes now have little or no trace of a bailey, and some further analysis of the simple groups A, B and C (without suffix) is necessary, if only to distinguish mottes from mounds of other periods, from tumuli to landscape gardening.⁶ A glance at a distribution map reveals several clusters of adjacent sites, but further examination shows that most apparent concentrations are chains of intervisible sites, over a mile apart, lining the passes through the hills.⁷ Although such chains had a common purpose, and formed a corporate defence line in time of danger,⁸ the individual castles were built as need arose, at different times and in different styles. Castles less than a mile apart may be due to one of two reasons—either one was set up in opposition to the other, or one supplanted the other.

Ideally, the siege castle was thrown up overlooking the besieged and just out of bowshot. Practical experiment shows that the longbow had an accurate range of some 150 yards, and the crossbow of 250 yards, with an extreme range of half as much again. The true longbow, however, does not appear before the late 13th century,⁹ and the crossbow, although in use

⁵ The pearshaped enclosure astride the causeway may be the castle of Alrehede, built by William I in 1070 to blockade Hereward (*Liber Eliensis* ii 245).

⁶ The cautionary tale of the mounds at Boughton in Kent should be read by every field archaeologist (*Arch. Cant.*, LXIV, pp. 35-8).

⁷ Two chains have been studied in detail—the vale of Montgomery by Miss L. F. Chitty (*Trans. Shropshire A.S.* LIII, pp. 83-90) and the Golden Valley by Mr G. Marshall (*Trans. Woolhope N.F.C.* 1938, pp. 141-58).

⁸ *Close Rolls*, II, 42 (1225).

⁹ *Antiquaries Journal*, XXIII, p. 54 (MS. of 1298). A 4-ft. yew bow from the inner east ditch of Berkhamsted Castle (*Op. cit.* XI, p. 423) is probably a relic of the siege of 1216 (*Op. cit.* III, pp. 37-42). The archer's pit at Clough (*U.J.A.*, 17, pp. 107-8), of c. 1200, suggests a bow fired at eye-level.

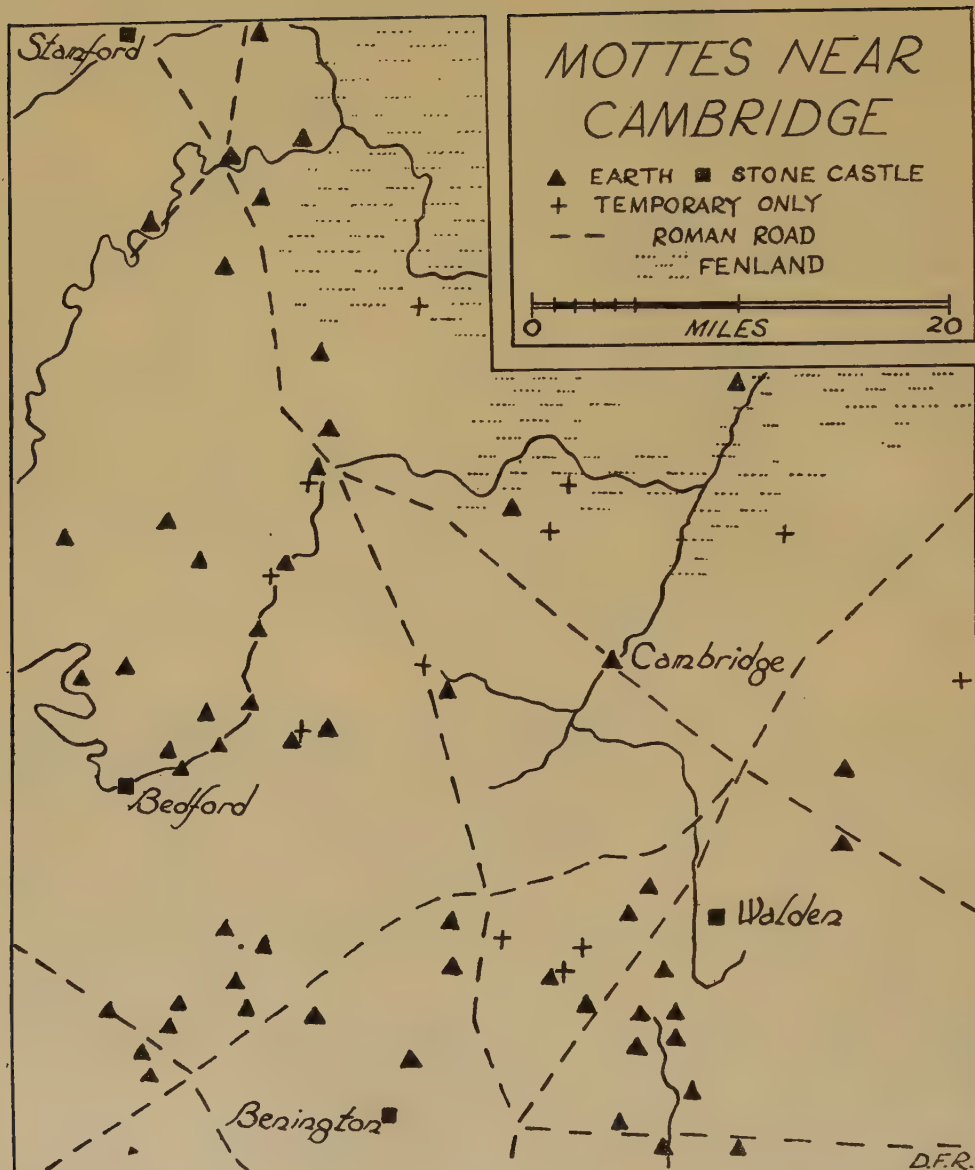


FIG. 2.

before 1137¹⁰ was effectively banned for fifty years by the Lateran Council in 1139 as a weapon hateful to God—a sobering thought in this day of ICBMs. The bow of the Bayeux tapestry was a short weapon fired from the side¹¹ and Mr H. L. Honeyman acutely observed that the axial length of many early motte and bailey castles was 100 paces, so that the bailey

¹⁰ Suger, *Gesta Ludovici*, 10. A crossbow nut and quarrel, excavated with early 12th-century pottery from Wareham Castle, must belong to the sieges of 1139-42 (William of Malmesbury, *Gesta Regum*, 522, 766-88). Crossbow quarrels occur on mid-13th-century sites (London Museum *Mediaeval Catalogue*, p. 68).

¹¹ Sir James Mann in *The Bayeux Tapestry* (Phaidon).



FIG. 3.

could be commanded from the motte.¹² The two principal mottes at Lewes are 200 yards apart, and thus could sweep the bailey between them; the second mound at Lincoln is only 100 yards from the motte and may only be part of the bailey bank, and no reliable plan of the double-motted castle at Ripon survives.

¹² *Proc. Soc. Ant. Newcastle-upon-Tyne*, 4th series, x, 294-8.

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Thus during the 150 years after 1066, the heyday of the earthwork castle, a siegework could be thrown up with ease at any distance over (say) 150 yards from the defenders. The list below of pairs of sites up to half a mile apart suggests that, other things being equal, a range of 200–300 yards was the optimum.

Many cases of one site being abandoned in favour of another are obvious, particularly where stone defences replaced earth. The palace at Huntly, Morpeth Castle and many another lie near disused mottes. But it is not so obvious when one earthwork replaced another; clues are supplied when one castle is obviously unfinished, or when both sites look in the same direction, the move being to a higher site across a stream or other boundary. A move of less than half a mile would usually be unprofitable—the lists show few pairs of sites between a quarter and a half mile apart.

Thus the proximity to another castle may explain the origin of over seventy mottes (a not inconsiderable fraction of the total so far recognized in England and Wales) spread throughout the length and breadth of the land (FIG. 3). In some cases a date of construction can also be proposed. The upper limit of a mile for the lists is arbitrary, for greater distances are known—the Arundel siegeworks, and the move from Old to New Buckenham, for example. But the greater the distance, the more speculative is such an explanation without documentary support.

INVENTORY OF TEMPORARY CASTLES IN ENGLAND AND WALES— I: SIEGEWORKS

Unless otherwise specified, the siege castle is a simple mound (A, B or C)

<i>Parish</i>	<i>County</i>	<i>Distance apart (yards)</i>	<i>Notes</i>
Sandy . . .	Beds.	650	On bank of hillfort overlooking castle.
Wallingford . .	Berks.	270	Built by Henry of Anjou in 1152, relieving castle from Stephen's counter fort at Crowmarsh.
Hay . . .	Breck.	250	Tower foundations on natural spur (scarped). Possibly due to Welsh rising of 1116.
Talgarth . . .		200	
Dolwyddelan . .	Carn.	300	
Aberystwyth . .	Cards.	800	
Lampeter . . .		800	Built in sieges of 1001, 1003, 1068 or 1069. Mound piled round keep of 1195. Compare mottes at Ascot d'Oilly, Bungay, Farnham and Wareham and church towers at Bleddfa and Cascob (Rads.).
Exeter . . .	Devon	Destroyed	
Lydford . . .		100	
Winkleigh . . .		250	Similar but opposing sites. Siege of 1139 (Type Cb3).
Chelborough . .	Dorset	150	
Corfe . . .		300	
St Osyth . . .	Essex	250	Built by Richard de Lacy in siege of 1174.
St Nicholas . .	Glam.	450	
Idsworth . . .	Hants.	150	
Aston . . .	Herefs.	230	
Dorstone . . .		500	
Eardisland . . .		170	
Shobdon . . .		190	
Huntingdon . .	Hunts.	270	

MOTTES: A CLASSIFICATION

<i>Parish</i>	<i>County</i>	<i>Distance apart (yards)</i>	<i>Notes</i>
Dingestow . .	Mon.	250	Siege of 1182.
Norwich . .	Norfolk	Destroyed	Built in siege of 1074.
Bamburgh . .	Northumb.	450	Built in siege of 1095.
Old Radnor . .	Rads.	400	
Bridgnorth . .	Salop.	350	Siege of 1101 (Type Cc2). Another Type C destroyed.
Wollaston . .		300	
Marston . .	Warws.	400	
Wanborough . .	Wilts.	200	
Northallerton . .	Yorks.	200	Possibly of 1174 (cf. Huntingdon).
Pickering . .		500	
York		300	Built after risings of 1068 and 1069.

INVENTORY OF TEMPORARY CASTLES IN ENGLAND AND WALES— II: RESITING

<i>Parish</i>	<i>County</i>	<i>Distance apart (yards)</i>	<i>Notes</i>
Penmon . .	Angl.	550	Mound on shore before 1086, matching site at Aber (Carn.). Castell Lleiniog (Type IIBc3) of 1096. ¹³
Alexanderstone . .	Breck.	800	
Talgarth . .		1300	
Hamstead Marshall	Berks.	30, 900	Unfinished motte, and two adjoining sites, one dismantled in 1233. ¹⁴
Great Hampden . .	Bucks.	30, 800	Two adjacent mounds (with ramps across ditch) at end of Grim's Ditch—surface finds of Roman and mediaeval pottery and slag. Third mound has a small bailey (crop-mark).
Little Kimble . .		700	Ploughed-out castle beside church produced early Norman scratch-marked pottery. Later finds from Cymbeline's Mount on the ridge above.
Llanedy . .	Carm.	} 600	Former site has square enclosure on opposite flank of meander 100 yards away. ¹⁵
Llanteilo . .	Glam.		
Pelynt . .	Corn.	1000	Two similar sites (Cd2) on parallel spurs.
Pentre Foelas . .	Denb.	1300	
Bridestowe . .	Devon	30	Large unfinished site close to motte with tiny bailey.

¹³ Square fort may have been built to defend the works at Beaumaris; both are of local stone, and the analogy of Edward I's peels in the Scottish lowlands may be quoted, also the remodelling of the mottes at Builth and Caernarvon (ANTIQUITY, xxvi, pp. 25-34).

¹⁴ *Trans. Newbury D.F.C.*, vi, pp. 114-26.

¹⁵ Mottes near a large enclosure, possibly for cattle occur also at Alderton, Northants., Hailes and Hawkridge, Som., and Topcliffe, Yorks. 'Guard-point Mounds,' tiny mottes, have been identified on manorial sites (*Surrey Arch. Coll.*, 53, pp. xxvii-viii).

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<i>Parish</i>	<i>County</i>	<i>Distance apart (yards)</i>	<i>Notes</i>
Wembworthy .		100	Very elongated site parallel to a more normal one.
Great Canfield .	Essex	900	Downhill move for water defences.
Elmdon . .		1000	
Hawarden . .	Flints.	1000	Trueman's Hill covers 'dead ground' of castle.
Penrice . .	Glam.	1100	
Powderham . .	Hants.	650	Opposing Barley Pound on the Harroway.
Almeley . .	Herefs.	730	Move from confluence site (Type Bc2) to Type Bd3.
Clyro . . .		1400	
Huntington . .		550, 1050	Type Bc3 to Type Ab2. Also Turret Tump to Castle Twts?
Lingen . . .		1200	Type Ac4 to Type Bd2.
Longtown . .		1300	
St. Devereux . .		1300	
Anstey . . .	Herts.	750	
Southoe . . .	Hunts.	750	Manor Farm pottery generally earlier than that from Town Orchard. ¹⁶
Bala	Mer.	1100 (× 4)	Chain of sites beside Dee valley.
Kerry	Mont.	900, 1100	Links along ridgeway.
Powis		1300	
Lilborne . . .	Northants.	1500	Type Ad3 to Bd4.
Ascot under Wychwood . . .	Oxon.	600	Sites in different manors.
Cregrina . . .	Rads.	300, 900	
Glasbury . . .		400	
Knighton . . .		500	
Llanelwydd . .		900	
Bettwys y Cwrn .	Salop	1300	
Chirbury . . .		1100	Two sites covering the old road to Montgomery.
Westbury . . .		1100	Type C site below Ad3 (Caus).
Womaston . . .		900	
Alfriston . . .	Sussex	300	Type IBd4 below the enigmatic 'Burlough Castle'. ¹⁷
Arundel . . .		1300	Type C site below Type Bd2—Siege of 1101? ¹⁸
Lewes		500	Altered site near Priory.
Fillongley . . .	Warws.	1100	
Kendal	Westm.	600	Type C site above and across river from Type A.
Bradfield . . .	Yorks.	700	

¹⁶ *Proc. Cambs. A.S.* xxxviii, pp. 158-63; *Proc. Cambs. & Hunts A.S.* 7, pp. 1-6. One of these sites is mentioned between 1140 and 1153 (*B. M. Add. Chart.* 11233).

¹⁷ *Sussex N. & Q.*, v, pp. 80-2, xiv, pp. 19-22. Pingsdorf-ware jug from 'Burstow Castle'.

¹⁸ Another at Rackham Bank from the same campaign (*Sussex Arch. Coll.*, lxxiii, pp. 162-82).

An Archaeological Study-tour in China, 1958

by JOSEPH NEEDHAM

Readers of ANTIQUITY will know of the work which Dr Joseph Needham, F.R.S., Reader in Biochemistry in the University of Cambridge and Fellow of Gonville and Caius College, is doing to bring the history of science, scientific thought and technology in China to the knowledge of the Western world. During the summer of 1958 he undertook a tour of historical and archaeological study in China as the guest of Academia Sinica, who accorded him the warmest welcome, and every facility. At our request he has written up this account of the outstanding experiences of his journey.

THE general situation in archaeology in China today is one of enormous expansion. Intensive training of specialists is going on, so that it is possible to have teams accompany the builders of the great public works (roads, railways, canals, etc.) in order to deal scientifically with any remains which may be unearthed. Every large city in China now has a good historical and archaeological museum and the number of these is rapidly being extended to the smaller towns as well.

After meeting once again the leading personalities of Academia Sinica in Peking and arranging the programme, we spent some time in the eastern cities of Chinan, Shanghai and Nanking, after which we flew up the Yangtze valley, stopping at Wuhan, to the province of Szechuan, where we worked for some time in Chungking and Chhêngtu. Then, crossing the Chhinling Shan (mountain-range) by air, we visited Sian and travelled far up the Old Silk Road to the Thousand-Buddha Caves at Tunhuang, which I had visited before in 1943. On the way back we saw many things in Lanchow, the capital of Kansu province, and paid a visit to the comparatively newly discovered cave temples at Maichishan. Then we returned to the east via Loyang and Chêngchow and spent another ten days in Peking. Finally, on the way home via Hongkong, we had the opportunity of several days' study in Kuangchow (Canton).

In Peking, we spent a good deal of time at the Archaeological Institute of Academia Sinica, directed by Dr Hsia Nai. Among the matters there discussed, amidst the numerous treasures which the Institute Museum has on show, were the recovery of cereal grains and other foods from vessels in tombs from the Chou period onwards (on which a special monograph will soon be appearing), and the *sets* of musical instruments from Shang and Chou periods which have recently been recovered. The musicological significance of these sets of instruments of different sizes has been expounded recently by Li Shun in his *Chung-Kuo Yin-Yo Shih*. We also discussed with Dr Hsia the question of the production of cast-iron tools in the Warring States and Han periods and the exact structure of the wheels of carriages and chariots in those times. We were particularly interested in this subject, since we have shown elsewhere from textual evidence¹ that the technique of 'dish' in wheel-making was known and employed in the Han period at least, although it is generally considered to have been an achievement of 16th-century European wheelwrights. Among the objects in the museum here are a series of very beautiful terra-cotta forts with model

¹ *Physis*, 1958, vol. 1.



Map to show main places included in Dr Needham's journey.

crossbowmen at each corner of the ramparts, and also bronze bowls from the Warring States period with iron legs.

Many days had to be devoted to the Imperial Palace Museum in Peking (the Ku Kung Po Wu Kuan) which, as is generally known, has a very large historical section. This is arranged as a teaching museum and is kept in beautiful order, the glass cases containing not only the original objects but also explanatory material on subjects such as bronze-casting, iron and steel technology and so on. One excellent feature of the museum here, as indeed throughout China nowadays, is the parallel exposition of textual evidence by placing within the glass

cases good editions of the most important relevant ancient and medieval books with passages marked so as to throw light on the objects displayed or the person about whom the vitrine is delivering its message. It was interesting to see certain changes of emphasis since 1952. At that time the only three ancient personalities emphasized were Kungshu Phan, the patron of all artisans and mechanics, Mo Ti, the philosopher of universal love, and Hsimên Pao, the humanitarian irrigation engineer. Now, however, Confucius (Khung Chhiu) has come back into prominence as an educationalist, together with Kuan Chung as an economist and an encourager of industry, and side by side with them Sun Wu the strategist. Beside these, the philosophers Mêng Kho (Mencius), Hsün Chhing, Lao Tzu and Chuang Chou are also given complimentary exposition. There is also a new vitrine for Chhü Yuan the poet. One outstanding feature of the historical exposé in this museum is the excellent emphasis placed on science and technology. Fields such as mathematics, astronomy, medicine, engineering and alchemy, which were not mentioned before at all, are now well covered.

Among the matters of technological interest one may find good models illustrating the varieties of Han trace-harness for horses, and an interesting exhibit on the history of the wheelbarrow. There can now be little doubt that the wheelbarrow is a Han invention, not merely one of the 3rd century A.D. as has previously been thought, and it certainly antedated the first use of wheelbarrows in Europe by more than one thousand years. The salt industry, recognized nowadays universally as the origin of all the world's deep borehole drilling, receives also considerable attention, the famous Han bricks of Phêngshan being reproduced to show the derricks and the boiling down of the salt with the aid of natural gas. A magnificent hall is consecrated to the Sung period, not in the galleries in the courtyard, but above on the top of the Wu Mên (the great Southern Gate of the Imperial Palace). Here we find an excellent series of exhibits concerning the origins of gunpowder. We have admirable reproductions of parts of the pictures of Chang Tsê-Tuan, in which we can see rudders on ships, cantilever bridges and the like, dating from the Sung period (10th to 13th centuries A.D.). Over the central part of the Wu Mên itself there is another, even more magnificent, hall, which with its flags of the Thai-phing and Boxer revolutionary armies is somewhat reminiscent of Westminster Abbey. A wealth of material is assembled about all the nationalist movements in Chinese history, but with particular attention to the 19th century.

At Chinan, the museum is located in some relatively modern temple buildings, which follow traditional Chinese architectural style, though largely constructed of reinforced concrete. This museum contains, besides the historical exhibits, a fine series of cases on the natural products and industries of Shantung province. In all these museums we paid special attention to those bronze vessels of the Chhin and Han which could have been used for alchemical experiments. Here, for example, one finds a beautiful *tsêng* steamer in bronze, dating from about 500 B.C., with a water-seal. Elsewhere we saw evidence of the use of the water-bath in Han times, constituting thus a parallel to, if not considerably antedating, the introduction of the water-bath in chemical operations by Mary the Jewess in Alexandria. Elsewhere again we saw very large cast-iron cauldrons of the Han period, which gave the impression of having been used for metallurgical operations. At Chinan there are a number of important stone reliefs, dating from the Later Han period, which have been only recently excavated. One of these shows a number of scenes of ancient textile technology which should considerably enlarge our knowledge of this. Another stone, from Yenchow, found in 1954, shows a particularly clear representation of a wheelwright's shop with the wheels in construction. This motif is parallel to one which was published long ago by Chavannes, and may be expected to throw more light on this technology also in Han times.³

³ See *Physis*, 1959.

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Here for the first time we came across a good deal of pottery piping. In other museums, especially in Sian, there is a good quantity of this now assembled, and it is therefore clear that the *shui tao* or water-conduits mentioned in the literature of Warring States and Han times were in fact made, and very beautifully made, of stoneware. We saw well-fitting flanged pipes, right-angle bends and annuli for lining wells. Perhaps the most astonishing exhibit at Chinan is the real Ming warship some 22 m. long, which was recovered from the mud near Liangshan Hsien in 1956. Dated objects, including the anchor, show that it was in use in the close neighbourhood of A.D. 1377, and it was probably a warship patrolling the Grand Canal. It had at least two masts, was equipped with cannon, and has the typical Chinese nautical construction of transom stem and stern, and the bulkhead hull. The wood is in good preservation.

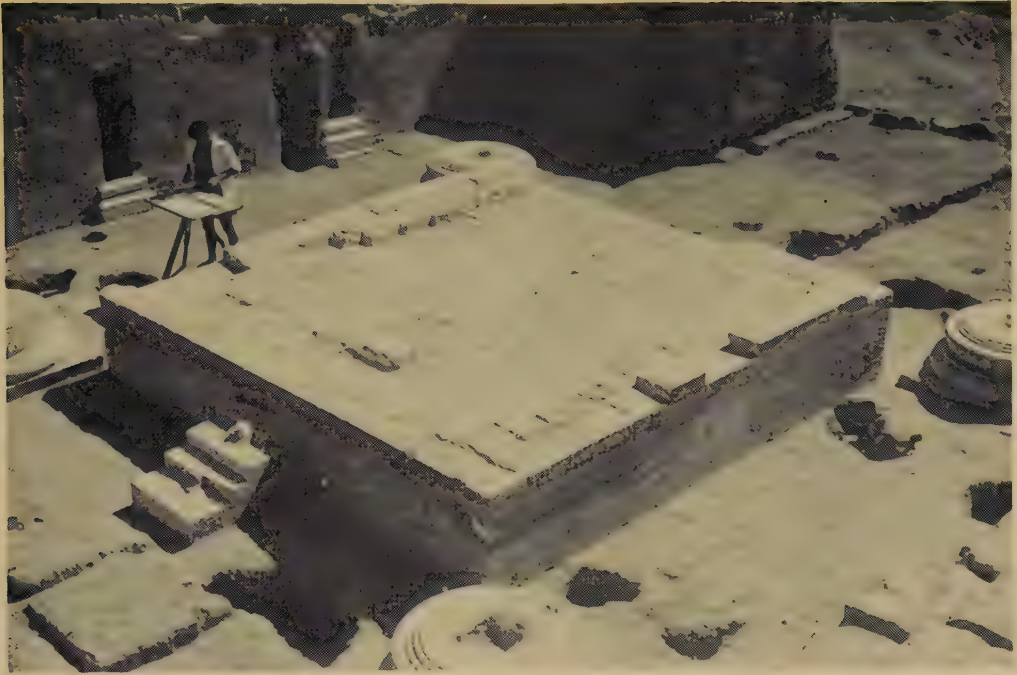
From Chinan we paid a special visit to the temple and tomb of Confucius at Chhüfou. The present head of the Khung family (descendant of the long line of titled representatives) is now the guest-master of the *chiao-tai-so* (guest-house) there, and shows visitors round the sacred sites with great charm. The gardens of the tomb, the pavilions where Emperors spent the night, the offering-halls and the great series of temple halls, are all in excellent preservation and carefully looked after. Much repainting and regilding has been done. One of the most interesting features of the central hall of the great Confucian temple is the presence of a complete orchestra of instruments for the old temple services. These musical instruments we carefully photographed.

The Nanking Museum is located in another building of reinforced concrete, but Chinese style, in the eastern part of the city. It is entirely consecrated to historical and archaeological objects, many of which are unique. For example, we saw there copper-reduction crucibles from the Shang period in refractory clay. These come from Anyang and would date from the neighbourhood of 1200 B.C. Very fine reliefs, showing ploughing and other agricultural scenes, have been newly discovered at Thêng-hsien and other places in Shantung, and a particularly interesting feature for hydraulic engineering is a Han model in terra-cotta of a dam and spill-way. In our volume on the history of astronomy in China,³ I have said that no representations of star-maps have so far been discovered in the cupolas of buildings such as are known in the Islamic world but this is no longer true because the Southern Thang tombs, excavated by the staff of this museum, yielded a very interesting astronomical map in the ceiling of the tomb of the Emperor Li Pien, who died in A.D. 943. Another important feature of this museum is the more than life-size bronze figure showing the acupuncture points, which was made in the Ming period, following closely the tradition established in the Sung—the oldest example of such a figure is in Japan, but reproductions of it are being made in China. In this connection it should be said that Shanghai possesses an admirable historical medical museum corresponding to the Wellcome Historical Medical Museum in London, though on a much smaller scale. This is well kept and organized, providing ample research facilities and a good library.

Prehistoric archaeologists will perhaps be interested by the presence in the Chungking Municipal Museum of a good deal of evidence for boat-burials. Quite a number of these burials in dugout canoes have now been found at Chaopei. These boat-burials date from the time of the States of Pa and Shu in the 4th century B.C. Among the many other interesting objects in this museum is a collection of very early porcelain, the *chhing tzhu* ware, previously known only from the literature. Now many beautiful pieces from the Six Dynasties period have been found in Szechuan during the building of the railway from Chungking to the north. They have a pale green glaze with a marked crackle. Among the objects in this pottery are curious economical lamps, which have underneath the saucer of oil a reservoir

³ *Science and Civilisation in China* (Cambridge), Vol. 3.

PLATE IX



SURKH KOTAL: (a) The great temple on the acropolis.
The *cella* with the four column-bases and the platform (1952).

[See p. 82



(b) The courtyard of the temple, being excavated, with part of the Western portico (1953).
From left to right (see FIG. 1): niches F, G, a tower, niche H, a doorway, niche E (the following tower and niches are still unexcavated).

[See p. 83



SURKH KOTAL: The acropolis. The hillside, with the temple, the three upper terraces, and the monumental stairway (1958).

See p. 84



SURKH KOTAL: (a) General view of the acropolis from the east (1958).

(b) The monumental stairway (1956).



SURKHI KOTAL: (a) The 'platform in the fields'.
A pilaster of the retaining wall.

See p. 84.



(b) Statue of a Kushan prince, from the acropolis.

[See p. 85]

PLATE XIII



FLINT MINE AT HOV: (a) A drift about 5 m. long in Shaft II, seen from the shaft. (The divisions of the pole are at 20 cm. intervals.)

See p. 89]



(b) Horizontal pole holes in the shaft wall, the last traces of a wooden platform.

See p. 92]

PLATE XIV



PELEUS AND ACHILLES: (a) Echinous. The acropolis from the south.

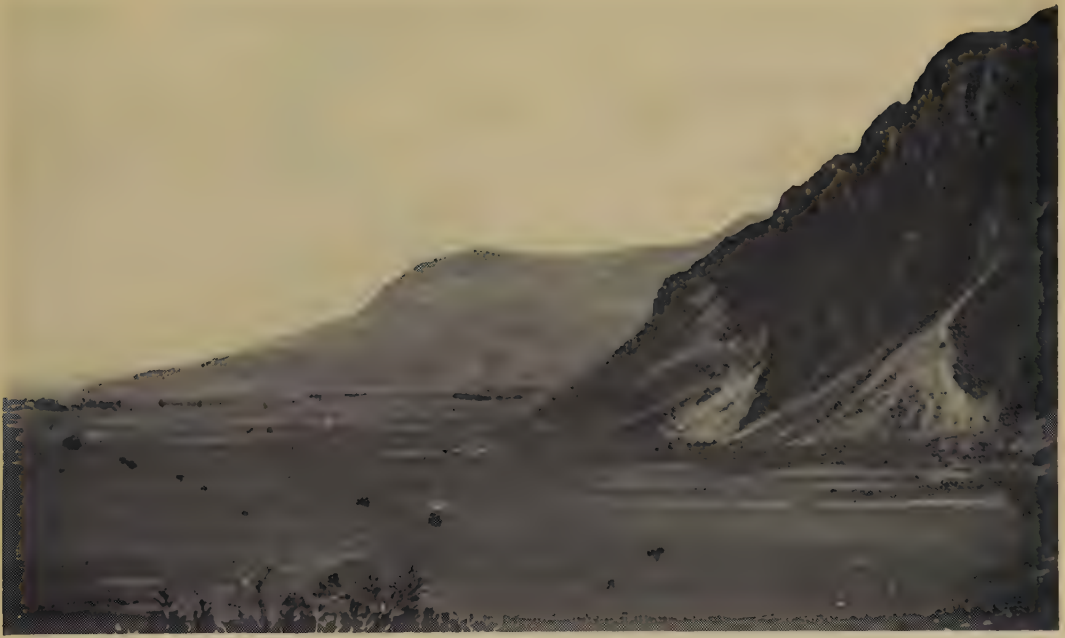
See p. 102]



(b) Platania. The site from the north.

See p. 103]

PLATE XV



PELEUS AND ACHILLES: (a) The Trachinian cliffs and Rakhita from the west.

See p. 104]



(b) Kastri. The site from the east.

See p. 104]

PLATE XVI



Plan of Dover Castle by H. Fouquet, 1737.

See p. 125]

(P.R.O., MPH. 335: by permission of the Keeper of the Public Records.)

for cold water to reduce the rate of evaporation of the oil. This is described in several old books, but no example was known until recently. A very curious bronze bowl, which may be from Thang or Sung, about 1 ft. 6 in. in diameter, has the property of raising a great spray of water into the air when the handles are rubbed in such a way as to vibrate the metal like a bell.

In Chhêngtu there are several museums. One, belonging to the University of Szechuan, possesses a number of interesting Tantric monuments and inscriptions and several representations on Buddhist reliefs of ships of the 5th and 6th centuries A.D.; another is the Municipal Historical Museum in one of the parks of the city. This latter has many interesting bricks from tombs showing agricultural operations, the milling of grain, winnowing, etc. A description of Chhêngtu would not be complete without mention of the Wang Chiang Lou park, beautifully kept up and sacred to the memory of a woman poet of the Thang dynasty, Hsüeh Thao. Even more remarkable is the votive temple to the great poet Tu Fu (the Tu Fu Tshao Thang) outside the city. Here in a shady temple compound with lotus pond, galleries, gardens and the like, there is a permanent museum of editions and translations of the works of the poet, a gallery of pictures illustrating his poems and a delightful tea hall.

Across the mountains at Sian there is, of course, the world-famous collections of inscribed stones, known as the forest of steles, the Pei Lin. Next door to this is the former Confucian Temple, which has been brilliantly restored and repainted and houses the Historical Museum. Many fine bronzes are to be seen here, and objects of technological interest, such as cast-iron sluice gates of the Thang period. An interesting magic square in Arabic figures was recovered from the foundations of the Yuan dynasty palace. It appears to have been buried between layers of stone as a talisman. The Archaeological Institute of Academia Sinica has a branch institute at Sian, where many interesting things are also on show. For example, there is a full collection of pieces from the tomb of Tuku Ssu-Chen, who died in A.D. 697. At Sian also, there is the Ta Yen pagoda, and around it the temple of Great Loving-Kindness where Hsüan-Chuang settled down to translate the *sūtras* after returning from India in the 7th century A.D.

In Lanchow, the Kansu Provincial Museum is established in the old Imperial Temple, which used to be a school when I was there in 1943. It is close to the Governor's Yamên. Here we saw cast-iron carriage axle bearings from the Han, bronze forks and an extremely beautiful complete bronze carriage and horse. Unfortunately there is no harness; it was probably of cords and leather and decayed with the centuries.

Horse harness and the history of animal traction turned out to be the chief point of interest for us at the Thousand-Buddha Caves near Tunhuang. Although I took many notes on other matters of technical interest, such as agriculture and shipping, from the enormous range of frescoes in these caves, we were able to pin down much closer than before the origin of the collar-harness of the horse. From the data now collected from Tunhuang one may place the appearance of this technique, so important in the history of the use of animal power, at somewhere in the near neighbourhood of A.D. 500. As is well known, it did not reach Europe until more than 500 years later than that. By contrast, the newly discovered cave-temples at Maichishan yielded less of technological interest, since the frescoes and statues are all more purely religious in character. However, it was a great pleasure to see the astonishing and magnificent cliff at this site honeycombed as it is with caves and connected by galleries. At both these places, but particularly at Tunhuang, the research archaeologists are accomplishing a splendid work of restoration by lighting the caves electrically, improving the approaches, adding doors and other protections, and in general doing everything possible to preserve these works of art for posterity.

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At Loyang the museum is located in the temple of the God of War, the Kuan Lin, some miles south of the city. Here is perhaps the most remarkable work of restoration which we saw anywhere in China. The elaborate wood-carving has been brilliantly restored, and the whole large compound brought to a perfect condition of preservation. This museum has a great many weapons from the Chou and Han, many Han iron tools and a fine stone inscription about the Imperial University of the Chin dynasty, dated A.D. 278. This tells us that there were more than 10,000 students, both from east of the sea and west of the shifting sands. Many of the students' names are given.

Chêngchow is now a very large city, which has burst the bounds of the old city wall and covers some 15 miles of country. The old city is being left to itself as a quiet reminder of the past. The museum here is associated with the Provincial Archaeological Institute and occupies a pavilion in a glade of a wood. It is rich in moulds for metal objects from the Shang, and here one may see remarkable pots used for the burial of children at that period. A series of bricks, hundreds of them, recovered from tombs at Têng-hsien, give us the most beautiful pictures yet seen of soldiers carrying standards and weapons, official personages with chalices on trays, musicians playing musical instruments, and caparisoned horses from the period of the Southern and Northern dynasties. They may be Northern Wei. But the most unexpected discoveries of this Museum relate to a large tomb of a prince of the Warring States period from the State of Chhu, excavated near Chhangtai-Kuan on the Huai river. Here the lacquered woodwork is quite extraordinary. There is a whole houseful of furniture with all kinds of pieces in perfectly preserved lacquer. There is a magnificent ring of bronze bells and lacquered musical instruments. Replicas of these have been played over the radio. Here the tubular frameworks for connecting together carriage canopies are particularly elaborate. But perhaps the most remarkable of all these discoveries is that of silks which appear to be of the polychrome drawloom type. It has not so far been considered that the drawloom was developed earlier than the Early Han, but here we have red and black figured silks which must have been woven in Chhu in the 4th century B.C.

Back in Peking, I had occasion to visit the art sections of the Imperial Palace Museum, where a good deal of archaeological material is also concentrated. For example, there is the great Shang chime-stone, about $3\frac{1}{2}$ ft. long, and the bronze helmet of the Shang period. The clepsydras in the Imperial Palace are in good condition and have recently been the subject of experimental research. Much interest has been created throughout the world in the systematic excavations which have begun at the Ming tombs some miles north of the city. We were privileged to be taken to the tomb of the Wan-Li Emperor (died A.D. 1620), which has been thoroughly excavated. Three very large halls were contrived about 60 ft. below the surface of the burial mound behind the range of temple halls, and there one may see the throne of the Emperor, his Empress and his principal Consort, together with the dais on which their coffins rested. Particularly striking are the doors of white jade about 12 ft. high, kept in position by colossal bronze lintels about the thickness of a man's body. It is the intention of the Chinese authorities to excavate all thirteen tombs of the Ming Emperors, but they began with this tomb (the Ting Ling) because evidence was available which enabled them to proceed most evenly with the excavation. The other tombs are scheduled for excavation later. In course of time they will surround a large artificial lake, the dam for which was constructed this summer by voluntary labour of Peking citizens. Though primarily for irrigation and power, it will turn the Ming Tomb area into a beauty-spot rivalling Hangchow.

Lastly, we had several days of work in Canton. With the aid of a launch placed at our disposal by Academia Sinica, we did our best to find and photograph examples of the treadmill-operated paddle-boats which used to ply regularly on the Pearl River until

recently, but unfortunately we were unsuccessful. They must all have been dismantled during the last few years. These paddle-boats are of considerable interest because they go back to the Thang period, at which time in China the first paddle-boats were actually built and used. However, in another aspect of nautical archaeology we were quite fortunate, because we were able to study the magnificent ship models dug out from Han tombs underneath the city itself during the course of recent renovations and buildings. The greatest of these models, about 20 in. long and 6 in. broad, is replete with technical detail, its most significant aspect being the presence of a distinctive rudder. This was held in place by tackle, the details of which are lost, but the shape of the rudder is perfectly clear and characteristic, and it may be considered the ancestor of all rudders. This particularly interested me because in the appropriate volume of our book⁴ an argument based on textual evidence had been elaborated to show that the origin of the stern-post rudder occurred in the Chinese culture-area, paradoxically in the absence of any true stern-posts but probably because of the existence of vertical members in the bulkhead construction to which such true rudders, as distinguished from the long steering-oars, could be attached. This new archaeological evidence which came to light about three years ago seems to prove the point without possibility of error.

The famous Canton water clock is now installed in the Museum and can be examined closely. As is well known, this dates from A.D. 1316 and we know the names of the artisans who made it, Tu Tzu-Shêng and Hsi Yün-Hsing. Besides this, there is a quantity of other objects of great interest in this museum, which occupies a striking tower-like building of four storeys on the top of the hill, the Yüeh Hsiu Shan, dominating the whole city. This building dates from A.D. 1380, though it has often subsequently been repaired.

It only remains to say once again that our warmest thanks are due to Academia Sinica for all the hospitality and facilities which they afforded to us. It seemed as if nothing could be too much trouble for them to do in aid of our enterprise of examining the history of science and technology in China, and setting it in its due place in the world history of man's knowledge of Nature and control over natural forces.

⁴ Vol. 4, pt. 2, Section on Nautics.

Book Chronicle—continued from p. 86

THE CITY AND UNIVERSITY OF CAMBRIDGE. Edited by JOHN ROACH, and being Volume III of the 'Victoria History of the County of Cambridge and the Isle of Ely'. pp. 524, 56 illustrations and 5 maps and plans. Published for the University of London Institute of Historical Research by the Oxford University Press, 1959. £7 7s. (cloth), £9 9s. (half-leather).

KHAMI RUINS. By K. R. ROBINSON with reports by G. BOND and E. VOCE. pp. 192, 50 text figures, 28 plates. Cambridge, The University Press, 1959. 40s. A report on excavations undertaken for the Commission for the Preservation of Natural and Historical Monuments and Relics, Southern Rhodesia, 1947-55.

LIFE AND DEATH IN THE BRONZE AGE. By SIR CYRIL FOX. pp. 193, 84 figures, 3 maps, 49 plates. Routledge and Kegan Paul, London, 1959. 45s. An account of one great British archaeologist's field work in western Britain in burial sites dating between 1700 and 700 B.C.

[continued on p. 120.]

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- LE COL ALPIN FRANCHI PAR HANNIBAL: SON IDENTIFICATION TOPOGRAPHIQUE. By M. A. DE LAVIS TRAFFORD. pp. 95, a map and 15 plates. *Librairie Termignon, Saint Jean-de-Maurienne, Savoie*, 1958. A fresh attempt to identify Hannibal's route.
- THE FOLKLORE OF BIRDS. By EDWARD A. ARMSTRONG. pp. 272, 85 text figures, 32 plates. *Collins, London*, 1958. 30s. Volume 39 in the 'New Naturalist' series. Subtitled 'An Enquiry into the Origin and Distribution of some Magico-Religious Traditions'. Starts with 'The Death of the Bird-Man at Lascaux'.
- A HISTORY OF SOUTH INDIA. By NILAKANTA SASTRI. pp. 508, 7 maps, 1 colour frontispiece, 20 plates. *Oxford University Press*, 1958. 21s. The second edition of a book first published in 1955. Surveys South India from prehistoric times to the fall of Vijayanagar.
- THE IDENTIFICATION OF NON-ARTIFACTUAL ARCHAEOLOGICAL MATERIALS. Edited by WALTER W. TAYLOR. pp. 64. *Washington*, 1957. Publication 565 of the National Research Council of the National Academy of Sciences. The report of the Conference held in Chicago in March 1956 on Archaeological Identification.
- JAPAN. By J. EDWARD KIDDER. pp. 282, 48 plates, 65 text figures and 7 maps. *Thames and Hudson, London*, 1959. 25s. The first overall up-to-date view of Japanese prehistory and protohistory in a Western language, this forms the tenth volume in the 'Ancient Peoples and Places' series.
- WINDMILL HILL AND AVEBURY. By ISOBEL SMITH. pp. 24. *Clover Press*, 1959. 5s. (Obtainable from *Barrie Books Ltd*, 2, *Clement's Inn, Strand, London, W.C.2.*) A generously illustrated short account of the 1925-39 excavations prepared in response to requests from visitors. A detailed report is in preparation by the author.
- A PROFILE OF PRIMITIVE CULTURE. By ELMAN R. SERVICE. pp. 474. *New York, Harper and Brothers*, 1958. \$6. Twenty portraits of communities varying from the Arunta and the Yaghan to the Ashanti and the Inca, and including four modern folk societies—in Yucatan, Morocco, China and India. Of great value to those who wish to interpret the past in terms of the present without making anachronistic judgements; to be set in the library alongside Daryll Forde's *Habitat, Economy and Society*.
- FAUX ET IMITATIONS DANS L'ART. By GUY ISNARD. pp. 324, 8 plates. *Paris, Librairie Arthème Fayard*, 1959. 1250 francs. The first volume of a two-volume work on frauds and forgeries; takes the story up to the 16th century. An extremely good first chapter on Les Faux Préhistoriques; clear summaries of Glozel and Piltdown, admirably restrained account of Rouffignac, though some must find the inclusion of this site, now open to the public and equipped with a railway, in such a book, a matter for heart burnings and searchings.
- THE CULT OF THE MOTHER GODDESS. By E. O. JAMES. pp. 300. *London, Thames and Hudson*, 1959. 35s. The theme demanded illustrations such as appeared in Dr O. G. S. Crawford's *The Eye Goddess*, and it is surprising that this book has not a single half-tone or line illustration.
- HELLENISM: THE HISTORY OF A CIVILISATION. By ARNOLD J. TOYNBEE. pp. 255, including two maps. *Oxford University Press*, 1959. 7s. 6d. Volume 238 in the Home University Library.
- THE PEOPLE OF THE DEAD SEA SCROLLS IN TEXT AND PICTURES. By J. M. ALLEGRO. pp. 51 and 147 pages of plates. *London, Routledge and Kegan Paul*, 1959. 30s.
- ARCHAEOLOGY AND THE OLD TESTAMENT. By JAMES B. PRITCHARD. pp. 263 including 77 illustrations and maps. *Princeton University Press (London, Oxford University Press)*, 1958. 30s.

Notes and News

L'INFORMATION ARCHÉOLOGIQUE

We welcome the first number of *L'Information archéologique*, published in January of this year; its price is 50 francs and it will appear every quarter. The annual subscription of 200 francs should be paid to the office of this enterprising paper at 25, *Faubourg du Temple, Paris X^e*. It describes itself as an 'Organe International de Liaison', is directed by A. Perraud; the present number contains details of forthcoming conferences and congresses, the discourse of Professor A. C. Blanc at the final session of the Hamburg Conference last August, a discussion of the place of archaeology by the Abbé Joly of Dijon, an obituary of Jean Jannoray and answers to general archaeological questions posed to a number of people. This four-page newspaper is cheap and worth encouraging. The second number published as these words go to press, will contain an article by Professor J. J. Hatt on 'Les caractères et les fonctions du Musée Archéologique.'

LONDON CONGRESS OF CLASSICAL STUDIES, 1959

The Third International Congress of Classical Studies will be held in London between 31 August and 5 September, 1959. The theme of the meeting is 'Tradition and Personal Achievement: New and Old in Classical Antiquity'. Lectures include Professor C. W. Blegen on 'The Palace of Nestor', Garcia y Bellido on 'The Problems of Iberian Art', E. Wistrand on 'Virgil's Palaces in the Aeneid', Professor G. E. Mylonas on 'The Walls and Gates of Mycenae', as well as five and a half hours of Mycenaean discussion. This year sees the bicentenary of the birth of Richard Porson, and the centenary of the birth of A. E. Housman, and both these events will be celebrated in the Congress. In association with the British Academy there will be a lecture on 'Porson' by Professor D. L. Page, and, in conjunction with University College, London, a Housman celebration lecture given by O. Skutsch. The address of the Secretary of the Conference: *Institute of Classical Studies, Fifth Floor, 31-34, Gordon Square, London, W.C.1.*

ANNUAL BIBLIOGRAPHY OF THE PALAEOLITHIC AND MESOLITHIC

There was published during 1958 the first volume of the *Bibliographie Annuelle de L'Age de la Pierre Taillée* (Paléolithique et Mésolithique) by the Union International des Sciences préhistoriques et protohistoriques, under the editorship of Professor R. Vaufray. This carries on from where the invaluable earlier bibliographies prepared by H. L. Movius came to an end. The first volume, as an exception, deals with 1955 and 1956: the second volume, which deals with 1957, is in the press at the moment, and the third, dealing with 1958, will be printed in March 1959. The price of these invaluable bibliographies is 1000 francs (or two dollars) for the first number, and 500 francs (one dollar) for the following volumes. This annual bibliography is sold by the *Service d'Information Géologique, B.R.G.G.M., 74, Rue de la Fédération, Paris, XV^e*.

A LATE BRONZE AGE WINE TRADE?

In a classic paper in *ANTIQUITY* thirty years ago, Mr de Navarro discussed the trade relations between Massilia and the Celtic world of Early La Tène times. He pointed out that among the objects of Mediterranean and classical origin finding their way into Central Europe from the 6th century B.C. onwards, 'most of the imported vessels, whether pottery or bronze, were ultimately connected with the carrying, serving, mixing and drinking of wine'—and in fact he went further, and in a now famous phrase roundly declared that 'La Tène art may largely have owed its existence to Celtic thirst'.¹ Jacobsthal made the same point in 1944, referring not only to the 'symposiastic outfit' comprised by stamnos, oinochoe and kylix, but drawing an apt parallel with the beginnings of the European trade in tea, when the introduction of a novel upper-class drink from China was accompanied by its appropriate porcelain vessels for infusing, pouring and drinking.² A similar accompaniment of wine with its traditional set of metal vessels for mixing, straining, pouring and drinking has been studied in its archaeological aspects in late La Tène times by Werner³ (a trade in which Belgic Britain participated), and within or beyond the Roman Empire by the same author,⁴ by Eggers⁵ and by Wheeler;⁶ finds of a similar nature in Britain were commented on by James Curle some years ago.⁷

In all these instances, ranging from the 6th century B.C. to at least the 3rd century A.D., we see the recurrent phenomenon of a wide dispersal of metal vessels which in its beginnings, at least, was the outcome of the trade in wine with the necessary equipment for its proper preparation and serving in the home of a barbarian aristocrat. Initially, the imported drink and the imported vessels would share the same prestige value, though this is not to say that the presence of the vessels would everywhere imply wine-drinking. Their own portability and durability would give them a wider currency than the exotic drink for which they were originally designed. Strainers would be necessary to deal with wine that had suffered the indignities, set on record by Pliny, of being mixed on occasion not only with sea-water or salt, but with pitch, pounded marble, or potters' earth,⁸ but in Denmark the Juellinge lady had, as Curle pointed out, used her Roman wine-strainer for berry-flavoured rye beer.⁹

It is now a commonplace of European prehistory that among the technological developments or innovations which serve as criteria in defining a 'Late Bronze Age' in Central and Northern Europe, one of the most significant is that of a command of the techniques of beaten bronze-work, largely expressed in a series of vessels of sheet metal. Handled cups in this technique go back to Reinecke's Phase D, and in the ensuing Hallstatt A and B phases a rich series of types are known in Central and Northern Europe, including various forms of bowls, 'buckets', cups, dippers and strainers.¹⁰ These types form, in fact, an important

¹ *ANTIQUITY*, II (1928), 423.

² *Early Celtic Art* (1944), I, 142.

³ *Bayerische Vorgeschichtsblätter*, XX (1954), 43; cf. my remarks on Belgic wine-parties in *ANTIQUITY*, XXII (1948), 22.

⁴ *Loc. cit.*; *Marburger Studien* (1938), 259.

⁵ H. J. Eggers, *Der römische Import im freien Germanien* (1951).

⁶ R. E. M. Wheeler, *Rome Beyond the Imperial Frontiers* (1954), 7-91.

⁷ *Proc. Soc. Ant. Scot.*, LXVI (1931-32), 307-10.

⁸ *Nat. Hist.*, XIV, 24, quoted by de Navarro, *loc. cit.* Dr D. Storm Rice draws my attention to wine-strainers in the early Islamic world, cf. his 'Deacon or Drink' in *Arabica* v (1958), 15.

⁹ *Loc. cit.*, 307.

¹⁰ Of the many studies, that of von Merhart in *Festschrift des R-G Zentralmuseums in Mainz*, II (1952), 1-71, has perhaps the best range of illustrations of most of the types in question; see also W. Coblentz in *Arbeits. u. Forschungsber. z. Sächs. Bodendenkmalpfl.*, 1950-51, 135; strainers also at Hart (*Bayer. Vorgesch.*, XXI (1956), 46), Langeneisling (*Germ.*, XXX (1952), 265), and other sites.

NOTES AND NEWS

archaeological constituent of the Urnfield Cultures, with a dispersal, presumably by trade of some kind, beyond the frontiers of these cultures to, for instance, Scandinavia: Hawkes and Margaret Smith have recently shown us how the British Isles shared, in a lesser and restricted degree, in this same pattern of trade.¹¹ Childe and von Merhart have demonstrated that the centres of production of this metalwork were indeed in Central Europe,¹² and on current dating we would have to place its beginnings not far from 1200 B.C.

Is it possible that, with the later instances just quoted in mind, we may see in this series of metalwork also the archaeological expression of a Late Bronze Age wine trade? The recurrence of the main types of vessel involved must surely have a significance: the 'buckets', with the related 'situlae'¹³ and 'amphorae', provide large containers of the type of stamnos or crater; the strainers must have strained some liquid, and the dippers and cups can hardly be for purposes other than serving and drinking it. What, then, is this drink demanding new and expensive forms of table-ware? Wine is not, of course, the only drink that could be so served, but the similarity in the range of types between the Late Bronze Age vessels under review and those known to be used for wine in later antiquity does strongly suggest that they were so related.

The centre of origin in South Central Europe demanded by the archaeological evidence would be consonant with local production of wine within the limit of effective growth of grapes for this purpose, usually placed around Lat. 50° N., but we cannot ignore the fact that the eastern Mediterranean may well be involved in the story. Childe in 1948¹⁴ looked to Minoan and Mycenaean prototypes for the Central European cups we have just discussed (though he was less sure a year later),¹⁵ and in the last year Hood, in publishing a L.M.I. gold cup from a grave near Knossos has made a comparison with a Slovakian Late Bronze Age bowl.¹⁶ Whatever the proximate source, we are dealing with metal-workers' techniques of wide dispersal and high antiquity in western Asia, Anatolia and the Levant, as well as in Crete and Greece. Indeed, it seems likely that more than one technical process, including that of lathe-cleaning or spinning, link this world with that of Central Europe¹⁷—if the two are directly related, chronology demands that the contacts would have to relate to the final phases of L.H. III or to the Sub-Mycenaean phase. Wine was, after all, an ancient drink in Egypt and the Levant at least, and seems to have been a Mycenaean product,¹⁸ and it is tempting to think that the new bronze vessels manufactured and traded in Continental Europe from the 12th century B.C. may not only have had a technical link with the Aegean or Levantine world in their mode of manufacture, but have owed their popularity, at least in part, to the civilizing qualities of their contents. Perhaps not only La Tène art, but the achievement of Late Bronze Age Europe, may owe something to thirst.

STUART PIGGOTT

¹¹ *Antiq. Journ.*, XXXVII (1957), 131.

¹² Von Merhart, *loc. cit.* and in other studies; Childe in *Acta Arch.*, XX (1949), 257.

¹³ Cf. Jacobsthal, *op. cit.* 137 on 'stamnos-situla' vessels.

¹⁴ *Proc. Prehist. Soc.* XIV (1948), 189.

¹⁵ *Acta Arch.*, *loc. cit.*

¹⁶ *Ann. Brit. School Athens*, LI (1958), 88.

¹⁷ Cups in the Dresden-Dobritz find (of cups, bowls, strainer and small 'bucket') were certainly so finished, and further evidence (with unfinished examples) comes from Riesa-Gröba in the same region (Coblenz, *loc. cit.*, and technical appendix by A. Pietsch). The characteristic central indentation made by the pin of the lathe chuck within the omphalos which these examples show appears also on the Knossos gold cup already mentioned, the 'Octopus' gold cup from Dendra, and probably also the Enkomi silver cup, referred to by Hood, *loc. cit.* 87. The technique is, of course, well-known in much later contexts (cf. *Ulster Journ. Arch.*, XX (1957), 85). I hope to discuss this matter more fully on another occasion.

¹⁸ M. Ventris and J. Chadwick, *Docs. in Myc. Greek* (1956), 130, arguing (partly on absence of strainers!) against Evans's case for beer-drinking Minoans.

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EXCAVATIONS AT FARNHAM CASTLE

Recently the Ministry of Works, in an official bulletin to the press, gave a brief account of its excavations at Farnham Castle, Surrey. The present note is prompted by the bulletin's headline—' *Unparalleled* ' *Method of Construction*—which conceals rather than displays the importance of its information.

In the text of the bulletin, the outstanding discovery resulting from the Ministry's excavations is described as follows:

A trench cut across the middle of the Keep revealed that at the centre were the foundations of a tower 50 ft. square, which had stood on an earthen mound about 90 ft. in diameter across the top. The mound and foundations of the tower had been built up together, probably c. 1138, a method of construction so far unparalleled in Britain.

This method of construction is not, in fact, unparalleled. Mr Jope first brought an instance of it to our notice in the report on his excavations at Ascot Doilly in Oxfordshire.¹ That there the stone tower and its encasing mound (of clay) were smaller than their counterparts at Farnham in no way affects the relevance of this instance.

The present writer has drawn attention elsewhere² to the connection between the procedure revealed at Ascot Doilly and the methods used in the construction of wooden motte-towers. They are linked by the common basic conception of the tower as the primary element, militarily and constructionally; a conception which is demonstrated in several forms of evidence. That the wooden towers were typically founded on ground level and had earthen mounds heaped up around them is shown by the Bayeux Tapestry's illustration of the building of Hastings Castle, and is confirmed by excavations in Denmark.³ Further confirmation is implicit in the carving of a stilted motte-tower on a capital from Westminster Hall and in the slightly later documents which attest prefabrication and transportation of wooden castles. Both the form and the practice require a more secure foundation than is afforded to a building merely placed on top of an unconsolidated mound.

The real importance of the Farnham discovery lies, therefore, in its *not* being unparalleled. It is significant as a further instance of a recognized technique.

Two existing hypotheses gain weight from the new instance. First, it enhances the possibly significant resemblance between the forms of the Roman signal-station and the medieval motte-castle. On the Roman *limes* both wooden and stone towers, founded on ground level, were set in small heaped-up mounds derived from circular enclosing ditches. Secondly, it enables us more confidently to question certain assumptions concerning the timber/stone sequence of building on mottes. In the past it has been maintained that stone buildings on mottes were necessarily preceded by timber constructions. Only a fully-consolidated mound could take the weight of a stone building placed on its top, it was argued, and a preliminary phase of timber building was the only expedient which allowed a castle to function while its mound consolidated. But now we can see that by use of the traditional method of motte-construction a stone tower could safely be set in place from the first; so this problem clearly need not have arisen where the original intention was to build in stone. We may therefore regard the use of timber castles in most cases as a matter of purely military choice or expediency, dictated by consideration of speed, economy and short-term needs. Wooden works can be put up more quickly than stone ones; and, while suitable timber would usually be available locally, the advantages of prefabrication were known and exploited.

¹ *Oxoniensia*, xi-xii, 1946-7, 165-71.

² 'The Excavation of a Motte at Abinger', *J.R.A.I.*, cvii, 1950, 42 f. *Recent Archaeological Excavations in Britain*, Routledge 1956, 247-8.

³ 'The Excavation of a Motte at Abinger', *J.R.A.I.*, 1950, 42.

The Ministry of Works is to be congratulated on the important results of its excavations at Farnham. Its further exploration of a well running through the body of the motte will be awaited with the greatest interest. The possibility that wells sunk in the natural ground might have had their linings extended vertically upwards, for incorporation during the construction of mottes, was suggested in a lecture to the Scottish Summer School of Archaeology in 1954. If the Farnham Castle well proves to have been made in this way, the excavations will after all have produced a feature without known parallel in the method of its construction.

BRIAN HOPE-TAYLOR

AN IRON-AGE HILL-FORT AT DOVER?

PLATE XVI

No fortification in the British Isles has a longer recorded history than Dover Castle. For, as Matthew Paris wrote in the reign of Henry III, 'it is the key of England',¹ and the chalk hill upon which the castle stands can show military works of every age from that of the Romans to the present day. Still in army occupation, it is only partially an Ancient Monument, and its defences, fascinating in their complexity, had never been systematically investigated until a detailed survey was recently undertaken by the Ministry of Works. The results of that survey will appear elsewhere. Meanwhile a reconsideration of the architectural history of the medieval castle has suggested a hypothesis which may be of interest to students of an even remoter epoch.

Before setting out that hypothesis it must be explained that to the historian of medieval military architecture the evolution of Dover Castle has always been difficult to follow, for it conforms neither to the standard 'motte and bailey' plan of the Normans nor to the 'concentric' plan of the Plantagenets, yet possesses elements of both (FIG. 1). Hitherto the accepted theory has envisaged as the primary fortification the oval earthwork encircling the Roman pharos and Saxon church. This was then extended northwards to form the site of the present Angevin keep with its encircling wall. Finally the outer curtain walls and towers were added by Henry III after the siege of 1216, in which the castle so nearly fell to the French. This sequence of events may be substantially correct, but it does not account for the outer earthworks. A glance at the plan will show that their relationship to the inner defences is curiously irregular, especially on the east, where they are interrupted, leaving a gap which created an awkward re-entrant in the outer curtain. It is hard to envisage any stage in the evolution of the medieval defences in which this feature would be explicable. Recently Mr Raleigh Radford has attempted to rationalize it by postulating a former inner bank and ditch parallel to the southern section of the western curtain (FIG. 1).² The two inner ditches (B B on his plan) would then form the 'bailey' of a Norman castle whose 'motte' is represented by the earthwork surrounding the site of the 12th-century keep. This theory, at first sight attractive, is vitiated by the fact that a careful scrutiny has failed to detect any sign of such an inner bank either on the ground, on aerial photographs, or on plans of the castle from the 16th century onwards, despite the fact that this part of the castle was free of buildings until the 19th century. We must therefore accept the early 18th-century survey (PLATE XVI) as a substantially accurate picture of the earthworks before they were altered by the military engineers of the late 18th and 19th centuries, and we must conclude:

(1) That they are not later than the 12th century, since it is now possible to demonstrate architecturally that substantial portions of the *outer* curtain wall in the northern part of the

¹ *Clavis enim Angliae est* (*Chron. Maj.*, Rolls Series, iii, p. 28).

² See the Official Guidebook (1953).

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DOVER CASTLE PLAN SHOWING SUCCESSIVE LINES OF FORTIFICATION-



FIG. I. Plan of Dover Castle from the Official Guide.

(By permission of the Ministry of Works.)

castle are in fact 12th-century work, and the curtain cannot have existed before the bank upon which it stands.

(2) That they are not contemporary with the 12th-century curtain, since they include a gap which the 12th-century engineers were at pains to close by a cross wall ending in a polygonal tower.

(3) That they are unlikely to be Norman, since they conform to no Norman layout that is consistent with the recorded facts of the site.

(4) That Mrs Armitage's theory that they were Saxon³ is contrary to all (admittedly not much) that we know about pre-Conquest fortification, and that there are no grounds for supposing that any part of the existing castle is Saxon work except the great oval earthwork round the pharos and Saxon church, which (in default of excavation) may be presumed to be the *castrum* which Harold threw up in the reign of Edward the Confessor.⁴

By a process of elimination, therefore, one is driven to consider the possibility of a prehistoric origin for the outer earthworks of Dover Castle—or rather to reconsider it, since G. T. Clark long ago gave it as his opinion that the castle stood on the site of a 'British camp' whose 'main ditch' had 'probably been deepened and more or less altered during the Norman period'.⁵ Here it must be admitted that no scrap of prehistoric pottery has so far been recovered from the site of the castle. But when it is remembered that most of the digging at Dover during the last century has been carried out by military engineers with no regard for medieval, let alone prehistoric, remains, this argument *e silentio* loses much of its force. No section has ever been cut across the main earthworks by a competent archaeologist (nor, it may be added, would this be an easy task in view of the later fortifications for which they have served as a foundation from medieval times onwards). Archaeologically speaking they are in fact virgin soil. We are left with their general character and their plan. Their form, in Mr Raleigh Radford's opinion, 'supports no attempt to see in the great banks and ditches of the castle a prehistoric hill fort'.⁶ But to other (admittedly less experienced) eyes their general character appears not inconsistent with that of other prehistoric earthworks (e.g. at Old Sarum⁷) similarly adapted to the purposes of medieval fortification, and in any case their profile has suffered so much from later alterations that it would be hazardous to draw any firm conclusion from it. The plan, however, remains, and the parallels which it offers to other archaeologically warranted hill-forts of the Iron Age are inescapable: first the site on a chalk eminence (cf. the Cité de Limes near Dieppe for a similar coastal site), then the close adherence to the contours, the large area enclosed (larger still before successive collapses of the cliff), and the indifference to a regular outline. To these may be added the puzzling gap in the eastern bank and ditch to which attention has already been drawn. Have we not here an overlapping entrance of the type recently planned and described by Sir Mortimer Wheeler at Fécamp (Camp du Canada) and at the *oppidum* of St. Désir near Lisieux?⁸ If so, the Dover hill-fort which has been postulated in this article would provide a further link between the British Iron Age and contemporary hill-forts in Belgic Gaul. Whether the comparison is valid, whether, indeed, the idea of a prehistoric hill-fort at Dover is more than a figment of a medievalist's imagination, it must be left to readers of *ANTIQUITY* to decide.⁹

H. M. COLVIN

³ E. S. Armitage, *Early Norman Castles* (1912), p. 139.

⁴ William of Poitiers, in *Scriptores Rerum Gestarum Willelmi Conquestoris*, ed. Giles (1845), pp. 108-9, 139-40.

⁵ *Mediaeval Military Architecture*, ii (1884), p. 12.

⁶ Official Guidebook, p. 3.

⁷ Where authenticated prehistoric pottery has recently been recovered from the outer banks by Mr P. Rahtz.

⁸ *Hill-Forts of Northern France* (Society of Antiquaries, 1957), pp. 63, 119.

⁹ I have to acknowledge the help in writing this note of Professor Stuart Piggott and of Mr A. J. Taylor, Deputy Chief Inspector of Ancient Monuments.

ARCHAEOLOGY IN WEST CORNWALL

Congratulations to the West Cornwall Field Club, who have produced *Gwithian: Ten Years Work (1949-1958)*, by Charles Thomas, and a twenty-fifth anniversary number of their *Proceedings* entitled *Archaeology in Cornwall, 1933-1958* and containing articles by Charles Thomas on 'The Palaeolithic and Mesolithic in Cornwall', J. V. S. Megaw on

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'Neolithic Cornwall', Bernard Wailes on 'The Bronze Age', A. M. Apsimon on 'Cornish Bronze Age Pottery', Miss Dudley on 'The Early Iron Age', C. A. Ralegh Radford on 'Roman Cornwall', Charles Thomas on 'Cornwall in the Dark Ages', and A. Guthrie on 'The Mediaeval Period in Cornwall'. These two publications can be obtained, post free, from the Treasurer, the West Cornwall Field Club, Penare House, Penzance; *Gwithian* for four shillings and the general survey for ten shillings. Both are extremely good value and represent the best traditions of regional British archaeology.

ROTARY QUERNS FROM INDIA

Dr H. D. Sankalia, Director of the Deccan College Post-Graduate and Research Institute at Poona, contributes the following note on rotary querns found at Nevasa. Brief reports on the excavations at Nevasa will be found in Indian Archaeology, 1954-5, 5, and 1955-6, 8.

Excavations at Nevasa, in the Ahmednagar District, have yielded several bee-hive type rotary querns or grinding mills which show affinities outside Europe. The evolution of the quern in India seems to be parallel to that observed in Europe. In all twelve complete pieces, besides broken fragments, were found and these are illustrated in the accompanying diagram (FIG. 1). Five of the upper stones and four of the lower were found stratified; all except two are of basaltic trap, the remaining two being of gritty sandstone.

These querns are quite unlike those in use in India today. Not only are the latter lighter, though bigger and broader, but all have a socket on the rim for a vertical handle, whereas the Nevasa querns have a transverse socket. Nothing like them has been found anywhere in India before. The nearest parallel is from Taxila, where Marshall found, for the first time, rotary querns from the late Saka-Parthian and slightly later periods of the type in use today. Along with these was discovered a rotary quern of a different type—heavy, with a groove or socket running through the whole length of the stone, which Marshall dates to the 5th century A.D.¹

Recently Dr M. G. Dikshit found over sixty pieces of the type represented in Nevasa 5a in his excavations at Sirpur, which were dated to the 6th/7th century A.D. This is probably the date of our 5a. Stratigraphically and typologically the Nevasa type (1-4 and 5) are the earliest. They were found on the floor of the earliest Andhra or Satavahana house. This immediately overlies a weathered layer, 2 or 3 ft. deep, of blackish soil; the latter in its turn seals a thick deposit of Chalcolithic culture. With the querns are associated a black-and-red pottery, a few sherds somewhat resembling Megarian ware (kindly identified by Sir Mortimer Wheeler and Dr Kenyon) and early Satavahana coins. The layers sealing this house yielded a Mediterranean amphora and red polished sherds, while the black and red ware is almost absent. Thus the Nevasa querns can be assigned to a period not later than the 2nd or 1st century B.C. According to the C-14 date, the layer above the earliest Andhra Shathavahan occupation is dated to 1846 ± 106 . Thus, on this evidence also the Nevasa querns can be dated to the earlier centuries before the Christian Era.

To satisfy our curiosity and that of a large number of visitors to our excavations, a lateral wooden handle was inserted, as well as a wooden pin, in the lower stone, and wheat and millet were ground on a complete quern. This gave a rather coarse flour, such as is used in certain dishes in India today.

The closest parallel outside India to these querns are among some of those described by Curwen.² If we disregard external form, the Nevasa rotary querns can be compared with the

¹ John Marshall, *Taxila*, (Cambridge, 1951), II, 487; III, 140.

² E. C. Curwen, *ANTIQUITY*, 1941, 17.

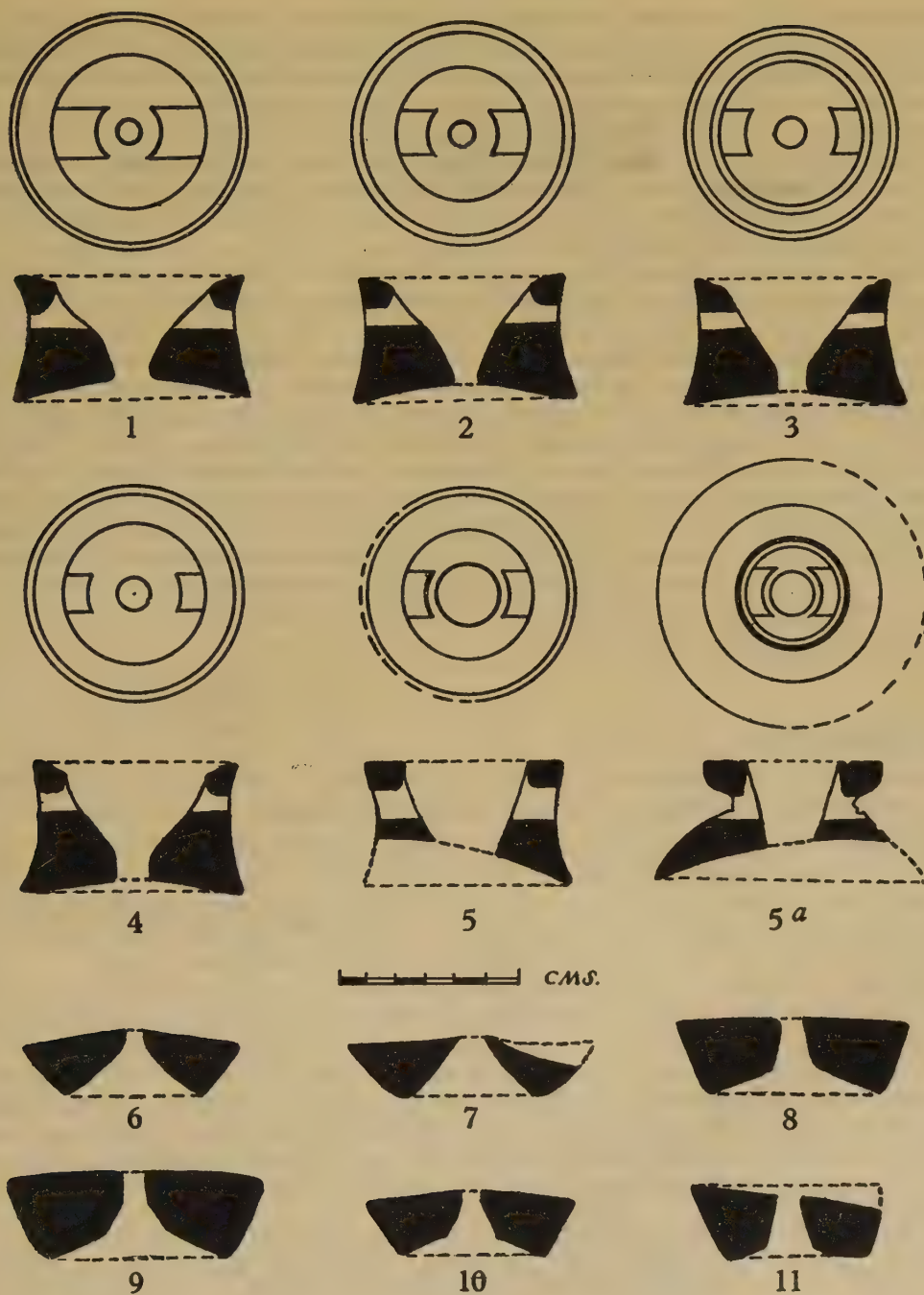


FIG. 1.

The twelve complete pieces of quern from the Nevasa excavations.

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Hunsbury type. But how did a quern type which had become familiar in the 1st and 2nd centuries B.C. in England and at least by the 2nd century B.C. in Greece and the islands, turn up in inland Deccan at about the same time? The absence of amphorae and red polished sherds from the early quern stratum at Nevasa seems to rule out the Roman trade which became intensive soon after 1 B.C. The presence of a few sherds resembling a Megarian bowl suggests that the west coast, like Arikamedu on the east coast, might have felt the effect of pre-Christian Roman influences. The late Professor Gordon Childe, on seeing these querns in our museum, suggested that the influence might be due to the Indo-Greeks, definitely in Western India by the 2nd century B.C.³

The subsequent evolution of this quern type also follows that in the west. Already at Nevasa (example 5a) we see that the upper stone has a deeply constricted waist. This type continues until today in some parts of Madhya Pradesh, so Dr Dikshit informs me. In one sense only did the evolution of this agricultural machine not follow the development in the west. In India a rotary quern has never been used commercially. Since its introduction into India in the 2nd century B.C., though we do have some very large and late specimens belonging to institutions, mass grinding of grain has been unknown in India. Until the introduction of power mills in cities and villages, *every* Indian household, however small or big, had its own rotary quern or querns. These were worked by the women folk, usually in the very early hours of the morning, and this practice has given rise to some charming and interesting folk songs. The reason why a rotary quern never became commercially important seems to be due to the existence of the caste-system. On the other hand, the manufacture of rotary querns, as of saddle querns, gave rise to a special class of artisans.

³ V. G. Childe, *ANTIQUITY*, 1941, 24.

THE MESOLITHIC

In this note Miss Judith Wilkins, of Girton College, a Research Student in the Faculty of Archaeology and Anthropology at Cambridge, discusses the origin of the term 'Mesolithic'.

It is accepted that those terms fundamental to prehistoric archaeology, 'Palaeolithic' and 'Neolithic', were devised by Sir John Lubbock, and first suggested in his book *Pre-Historic Times*, of 1865. In this he pronounced that 'it would appear that Pre-historic archaeology may be divided into four great epochs'; the first, the period of the Drift, or the 'Palaeolithic' age, and the second the 'Neolithic', or the age of polished stone.

The term 'Mesolithic', on the other hand, has been considered to be of more recent origin; and it has been thought that the present tripartite division of the prehistoric stone age was not suggested until twenty-seven years after the publication of *Pre-Historic Times*. The Oxford English Dictionary of 1933 has possibly led other authorities to ascribe the first use of the word 'Mesolithic' to J. Allen Brown. In a paper read to the Anthropological Institute early in 1892¹ he described certain flint artifacts and said that 'it is to these forms which appear to be of transitional age, that I would apply the term Mesolithic', and continued to suggest a fourfold division of the stone age into Eolithic, Palaeolithic, Mesolithic and Neolithic.

Brown's phrases do not seem to indicate any suspicion that he had been anticipated. Actually the term 'Mesolithic' had been coined only a year after the publication of *Pre-Historic Times*. Addressing the Anthropological Society on 15 May, 1866,² on

¹ *J.R.A.I.*, xxii, p. 66.

² *Anthropological Review*, 1866, p. clxxxiii.

NOTES AND NEWS

'Analagous Forms of Implements among Early and Primitive Races', Mr Hodder M. Westropp had proposed to distinguish prehistoric stone artifacts by the following terms: Palaeolithic, Mesolithic and Kainolithic, or recent stone age.

Later he elaborated this idea in the intriguing manner shown by the table which was published in 1872 at the beginning of his book, *Prehistoric Phases, or Introductory Essays in Prehistoric Archaeology*. The dedication, to Lubbock, indicates Westropp's admiration for a master in the field, and also perhaps the inspirer of certain of his own ideas.

Unfortunately, Westropp's book does not supply very much factual justification for the characteristics he ascribes to the various stages in the development of man. In general, much of his archaeology consisted of inferences from his ethnological knowledge; in a paper of 1867 life in the Mesolithic, or 'hunting stage', was compared with that of the American Indian.

He termed certain industries Mesolithic in order to distinguish them from, on the one hand, those now designated as Lower Palaeolithic, and on the other, the polished tools of the Neolithic; thus his intermediate period included not only the Danish Kitchen Middens, but also Lartet and Christy's Upper Palaeolithic finds from the Dordogne.

A firm and repetitive believer in 'the intellectual unity of the human race', Westropp published some ten books between about 1856 and 1885, on subjects ranging from *Homeric Doubts* to *A Manual of Precious Stones and Antique Gems*, and lectured to the Anthropological and Ethnological Societies upon varying topics, including 'The Mythic Age', 'Cromlechs and Megalithic Structures' and 'Phallic Worship'.

A lack of archaeological experience did not prevent his making some sagacious observations. He realized 'that the adoption of metal, however, was neither sudden nor universal' and he advised the prehistorian to be aware of 'giving the name of distinct peoples to the different phases of the same people'.

Perhaps it is surprising that Hodder M. Westropp and the term he apparently invented should have been so quickly forgotten. But the theory of the 'hiatus' between the Palaeolithic and the Neolithic was supported vehemently by such authorities as Boyd Dawkins, G. de Mortillet and Cartailhac; and until Piette's excavations at Mas d'Azil had yielded in 1895 what Clark³ has described as 'the famous section' that 'in itself destroyed the hiatus theory', perhaps no archaeologist was quite certain as to what 'Mesolithic' really meant.

³ J. G. D. Clark, *The Mesolithic Age in Britain*, 1932, p. i.

OREOPITHECUS BAMBOLII

In this note, Dr J. C. Trevor, Lecturer in Anthropology and Director of the Duckworth Laboratory in the University of Cambridge, discusses whether the recent discoveries in the Baccinello colliery in Tuscany are those of a Lower Pliocene hominid.

Shortly before 2 a.m. on 2 August, 1958, two Italian miners, Azelio Giustarini and Enzo Bocalini, were drilling underneath a loose block of lignite at the Baccinello colliery, 15 miles east of Grosseto, the capital of the province of that name in Tuscany. The block fell and revealed in the roof of the gallery where they were working—about 600 ft. below the present land surface—what is now known to be the greater part of the skeleton of a 12-million-year-old catarrhine primate. Of undoubted Pontian age (i.e. Lower Pliocene or, according to French authors, Upper Miocene), this enigmatic creature was called

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*Oreopithecus bambolii*¹ by Paul Gervais, who in 1872, on the basis of a lower jaw, described it as a small pongid related to the gorilla but having certain affinities with the cercopithecoid macaques.²

In 1880 C. I. Forsyth Major, in a footnote to a paper on fossil horses, recognized in the anterior (presumably lower) premolars of *Oreopithecus* an analogy to those of man, and stated that he would shortly publish an account of its dentition, for which adequate material was available to him. The son of a scholastic divine, Forsyth Major lived another forty-three years, but his promised monograph, which might have had disturbing implications for the problem of human origins eight decades ago, was never to appear.

For three-quarters of a century a number of divergent views were expressed concerning the systematic position of *Oreopithecus*, all of them dependent on the not very satisfactory evidence of casts. Apart from Rüttimeyer, who regarded it as a gibbon, and Winge, who had few doubts that it was an Old World monkey, most workers seem to have been perplexed by the mosaic of apparently cercopithecoid and pongid features shown in its teeth. Thus Schwalbe went so far as to create a separate family of apes, the Oreopithecidae, for its reception; and Schlosser and Gregory, both former adherents of the cercopithecoid thesis, eventually accepted *Oreopithecus* as a transitional form between cercopithecoids and pongids, Gregory further remarking that it might well be 'a unique survivor from a basal catarrhine stock'. However, owing to its extremely dubious zoological status, it was often overlooked in discussions of primate phylogeny.

In 1948 the distinguished palaeontologist Dr Johannes Hürzeler, Curator of the osteological collections at the Basle Natural History Museum, began a detailed re-examination of the original remains of *Oreopithecus* preserved in the University of Florence. This led him, the next year, to place it unequivocally among the hominoids and then, in June 1954, to make the revolutionary pronouncement that *Oreopithecus* had in certain respects become so differentiated in a hominid direction that it could be assigned to the Hominidae *sensu lato*. In the spring of 1956, with financial support from Swiss friends and the Wenner-Gren Foundation of New York, Hürzeler started excavations at the Baccinello Mine, which had yielded fresh *Oreopithecus* specimens. These he embodied in an admirably reasoned preliminary report on the material as a whole, published in trenchant English early in 1958. The following summer his ten years' labours were crowned by the recovery from the Baccinello lignite deposits of the almost intact skeleton of what an eminent American colleague has aptly, if irreverently, named 'The Abominable Coalman'. Thanks to the co-operation of the Italian authorities, who have made a gesture befitting the fellow-countrymen of Vesalius, Galileo, and Malpighi, this precious relic has been sent on loan to Switzerland, where the delicate task of freeing the bones from their matrix and then studying them is now being undertaken by Hürzeler and his associates.

When found, much of the skeleton was too deeply embedded in lignite to be seen, but the pelvic region, which, together with portions of the upper and lower limbs and some ribs, was visible, shows its owner to have been a tailless animal with a short sacrum and broad,

¹ The first part of the name of the genus was chosen to indicate the 'hillocky' nature of the high-cusped molar teeth of its members, and not to suggest that they lived in mountains! Indeed their habitat was paludal, a happy circumstance in palaeontology, for, while running water disperses and mixes animal remains, marshes tend to preserve these more or less intact at the scene of the owner's death. Monte Bamboli (also in the Tuscan *maremma*), the site of the earliest *Oreopithecus* find almost a century ago, accounts for the specific epithet.

² Simpson's scheme of the higher taxonomic categories of the primates, which accords respective super-familial rank as the Cercopithecoidea and Hominoidea to the Old World monkeys on the one hand and to the apes (Pongidae) and man and his predecessors (Hominidae) on the other, is adopted here because of its wide currency rather than as a *pis-aller*. The older term 'catarrhine', embracing both cercopithecoids and hominoids, is used infraordinally to distinguish them from the platyrrhine or New World monkeys.

flaring hip-bones quite unlike those of an ape. The previously-known characters used by Hürzeler for his diagnosis of *Oreopithecus* include a reduced upper facial skeleton, the steep symphysis of the mandible, which is chinless, but in the body of which the mental foramina are placed higher than among monkeys and apes, and hominid traits in such features of the dentition as the nature of the wear on the vertically-implanted incisors, the small canines without mesial or distal diastemata, the homomorphic mandibular premolars, of which the first pair are bicuspid and not sectorial, the form of the *fovea anterior* of the maxillary molars, and the proportionate lengths of the canines, premolars, and molars in both jaws.

Contrary to assertions in the popular press, Hürzeler has never alleged that *Oreopithecus* is directly ancestral to man but rather that, on the available evidence, it represents an extinct branch of the Hominidae with already such well-developed traits of its own as to suggest that hominoids bifurcated into hominids and apes in pre-Miocene times, each group thereafter pursuing a fundamentally different phylogenetic trend. This last inference brings Hürzeler fairly close to Osborn, who in 1927 abandoned his former support of the 'ape-human ancestry' hypothesis and declared his advocacy of 'an Oligocene neutral stock' from which hominids and pongids evolved independently. In any event, recent doubts on the part of Hürzeler and others concerning the primate nature of the supposedly hominoid *Parapithecus* and cercopithecoid *Apidium* and *Moeripithecus* from the Egyptian Oligocene and of the 'simian' (as opposed to tarsoid) *Amphipithecus* from the Eocene of Burma, all buttresses of orthodox theory, call for a reorientation of thought on the rise of the higher primates in general.

To return, however, to *Oreopithecus*, not unnaturally Hürzeler's assessment of its hominid status has not passed unchallenged, even among some who, like him, have worked on actual specimens. For example, Butler and Miles in their forthcoming study of a maxilla in the British Museum (Natural History) assert that the teeth of *Oreopithecus* do not fall within the range of variation of the living hominids, pongids, or cercopithecoids, and that the peculiar combination of primitive and specialized dental characters this primate displays suggests that it is a terminal form of a separate hominoid stock extending back probably to Oligocene times. Thus in Butler and Miles's view, while *Oreopithecus* may be regarded as belonging to the Hominoidea, no special relationship exists between it and the Hominidae. According to Straus, who is investigating the post-cranial bones of *Oreopithecus*, when their elements are taken together the characters of a generalized hominoid predominate. Nevertheless certain hominoid and cercopithecoid features also occur, and if the cercopithecoid ones are considered to be due to a primitive catarrhine heritage (as may well be argued), then the picture emerges of a hominoid with a sprinkling of hominid specializations which would not be incompatible with what might reasonably be expected in an early representative of the Hominidae. In other words, Straus's findings on the post-cranial skeleton confirm Hürzeler's on the skull and dentition.

Although the new evidence patiently being revealed in Basle will doubtless remove many hesitations, it must be conceded that the case already argued by Hürzeler is a strong one, resting, as it does, not on a few fragments but on the remains of about three score individuals. If, as would appear, *Oreopithecus* proves to be a Tertiary hominid, then it is the first of its kind and is a dozen times older than the hitherto most ancient claimants to inclusion within the human family, the fossil Australopithecinae of South Africa.

The writer, who visited Switzerland and Italy in the autumn of 1958, is greatly indebted to Dr Hürzeler for much kindness in showing him the *Oreopithecus* material brought together in Basle and in clarifying several points in relation to it. He is also grateful to Dr R. Egloff of Basle, Signor Giovanni Lorenz of Milan, Ingegnere Remo Minucci of the Baccinello Mine, Professor Adolf H. Schultz of Zurich, and Professoressa Maria Seri of

Grosseto for assistance of various kinds, and to Dr P. M. Butler and Professor J. R. E. Miles of the University of London and Professor William L. Straus, Jr., of The Johns Hopkins University, Baltimore, for leave to quote their findings in advance of publication.

J. C. TREVOR

ROUFFIGNAC AGAIN

The end of *la guerre des mammouths* is still, it seems to us, far away, despite the remarkable testimony published recently in a letter by Monsieur William Martin (*Sunday Times*, 1 March, 1959), in which he declared that there were no paintings at Rouffignac in September 1948, that two rhinoceroses appeared between then and December 1948, and the third by Easter 1949; and that all the paintings are works of the last ten years. France seems divided into two camps on this important issue: in the current number of *L'Anthropologie* Professor Vaufrey says of the Rouffignac paintings 'leur authenticité n'est pas douteuse' (*L'Anthropologie*, 1959, 370), but in the same week another most distinguished French archaeologist writes: 'The advertisement of *Rouffignac* in ANTIQUITY, March, 1959, is enough to make our old friend Crawford turn in his grave. You know that Rouffignac is considered a scandalous fake. This will be another Glozel.'

We print here two interesting documents relating to the present dispute. The first is a résumé of an article in the *Bulletin de la Société Préhistorique Française*, Vol. LV, fasc. 10, 588-92; the second an argument for the authenticity by Professor Paolo Graziosi.

I. ROUFFIGNAC: TEXTS AND PLANS

In *La Guerre des Mammouths*, Professor Nougier and M. Robert cite in support of the authenticity of the Rouffignac paintings written sources from the 16th century to the present day. These texts fall under four headings: the *Cosmographie* of François de Belleforest (1575) and its variants; plans; guides; eye-witness accounts from the end of the 18th century onwards.

The description of Miremont in Belleforest's *Cosmographie* is evidently drawn from a local source; it is perfectly clear that Belleforest himself, dubbed by Nougier and Robert 'first speleologist and first prehistorian', never visited the cave and is writing from hearsay. His mention of altars, mosaics and paintings forms part of the jargon which was used at the time in descriptions of this sort. He speaks also of 'tracks of different kinds of beasts', but later writers, in reproducing his text, have substituted the word 'vestiges' for tracks, until in the 17th century, in a document wrongly attributed to Chanoine Tarde, we get the version '... altars and paintings in various places, showing also vestiges of all kinds of beasts'. The presence of a comma between 'places' and 'showing' proves that there is no connection between the 'paintings' and the 'vestiges', but in the heat of controversy this comma has sometimes been omitted.

Belleforest's text was followed, with variants, all through the 17th and 18th centuries by authors who had nothing new to add. Nougier and Robert, while admitting that these authors derive from Belleforest, continue nevertheless to treat them as so many independent witnesses, giving thereby the impression of an imposing mass of testimony.

The first account which is admittedly that of an eye-witness comes in the early 18th century from the Marquis de Miremont, owner of the cave, who writes to a correspondent: 'All the ceilings are beautiful, but they are not all of the same height, and there are on them a number of beautiful paintings, and quantities of statues formed by drops of water, from which it appears that the whole is natural, without any appearance of having been touched by the hand of man.' Nougier and Robert quote this sentence as far as 'beautiful paintings',

and some lines further on add that the Marquis ' evokes with pleasure statues formed, apparently, by drops of water . . . ', but in thus dissociating the text they have vitiated its meaning.

Among 19th-century investigators the name of Martel stands out. He explored Rouffignac on 12 and 13 August, 1893, drew up a plan with his collaborators Rupin and the Lalandes, and described the cave in detail in his book *Les Abîmes* (1894). The fact that Martel nowhere alludes to any paintings calls for an explanation. Nougier and Robert provide one. E. A. Martel, they say, saw the Rouffignac paintings, and they point to the word *foirail* (market-place), printed on his plan opposite the wall on which is now the frieze of three rhinoceroses. They argue that Martel did indeed see these paintings, but that rather than engage in a new ' battle of Altamira ' he contented himself with taking note of them by writing *foirail* on his plan. Martel, however, was not the first to use this word in connection with Rouffignac. The *Guide Joanne, Paris à Agen, 1867*, speaks of a ' Foirail ou Place du Marché ' in the cave, and at an earlier date two visitors, Depping and Delfau, both speak of a *Place du Marché*, adding that it is easy to understand why it is so called when one notes that the damp soil has preserved the traces and footprints of all who have passed that way. Nougier and Robert quote Delfau without, apparently, seeing the bearing of this passage, doubtless because the position of the *Place du Marché* does not seem to correspond with that of Martel's *Foirail*. An 18th-century plan by Gabriel Bouquier clears up this question. As reproduced by Nougier and Robert, its legend is illegible, but reference to the original, in the Périgueux Museum, shows the *Allée du Marché* clearly marked, with one end corresponding to the point at which Martel had written *foirail*.

Finally, Martel, in his book *La France ignorée* (1930), recalls his visit to Rouffignac. Palaeolithic art is now an accepted fact, and he describes the paintings of La Mouthe, Pech Merle, Altamira, etc., but of Rouffignac he says: ' In short all the interest of the Cro de Granville is exclusively geological.'

Martel, then, did not see the Rouffignac paintings.

The attempt to provide historical evidence for the existence of Palaeolithic paintings in this cave is built on sand, but in discussing the texts I approach only indirectly the question of authenticity, which I do not claim to be able to decide.

S. DE ST MATHURIN

2. THE AUTHENTICITY OF ROUFFIGNAC

Professor Paolo Graziosi, Professor of Anthropology and Prehistory in the University of Florence, has allowed us to publish the following shortened version of a longer article.

After the Abbé Henri Breuil, Membre de l'Institut, the greatest living expert on Prehistory, had declared the paintings of Rouffignac to be authentic, many distinguished archaeologists from various countries, after a memorable visit to the cave in 1956, found no evidence of non-authenticity of the above-mentioned paintings. Amongst them were Martin Almagro of the University of Madrid, Leroi-Gourhan of the Sorbonne, Grassé of the Académie des Sciences, A. Beltrand of the University of Saragoza, Ripoll-Perelló of the University of Barcelona, Jordà Cerda of the University of Oviedo, L. Zotz and G. Freund of the University of Erlangen, H. Danthine of the University of Liège, A. Althin of the University of Lund, the well-known speleologist Norbert Casteret and others, including myself. Moreover, most of them pointed out the beauty of these paintings and the interesting part they played in the general picture of Palaeolithic art.

Dr Daniel has described¹ the Rouffignac paintings as ' dull, lifeless, uninspired and

¹ *Sunday Times*, November 23, 1958.

lacking the genius and originality of the artists of Lascaux and Font-de-Gaume'. We can agree in part with this statement because undoubtedly in these two caves we find the most magnificent examples of Palaeolithic art, hardly ever equalled by other wall paintings and engravings of the same period. There are in fact many other drawings which do not reach this same perfection and appear more harsh and awkward in design. It is sufficient to mention the horses with dots of Pech-Merle, the paintings of Marsoulas, some of the paintings of Castillo, of Santimamiñe, of Portel.

'It is no use saying that no one has the skill to copy Upper Palaeolithic paintings,' Dr Daniel goes on to say. 'In the little museum in the Château Fort of Reignac there are some very lively reproductions of Palaeolithic art done by an English artist called Henghes; therefore it would have been possible to paint fake pictures in the cave of Rouffignac.' I can also agree up to a point with this; but, reasoning this way, one could begin to doubt that even Lascaux was genuine. Only the trained eye of a specialist can separate the authentic from the false; otherwise museums and galleries all over the world would be filled with reproductions of the work of great artists.²

We now come to the crucial point of the matter: Pierret and Martin affirm that the paintings appeared in the cave *after* September 1948, and it would seem that some people who had previously visited the cave had seen nothing. It is most probable that many of those who entered the cave, including the above-mentioned speleologists, did not see the paintings because they did not reach the very deep corridors where these paintings are. Alternatively, they did not notice them either because of the many modern inscriptions which in some places cover the paintings, or because, as often happens, when one is not on the lookout or prepared to see something, one does not see it, or even if one does no importance is given to it.

On the other hand, it is a misstatement to say that nobody had drawn attention to them; it is well known that various writers between the 16th and the 18th centuries, such as François Belleforest in 1575, Gavignien d'Alquici in 1670, le Marquis de Miremont in 1715, the Abbé Morell in 1725, had mentioned the existence of paintings in the cave. It may be, of course, that these statements, which are rather vague, may have been influenced by that baroque desire for the fanciful peculiar to those times. But I do not believe that these statements should be completely set aside, especially as, in various parts of the cave, paintings can be seen covered by names and dates which undoubtedly belong to the same authors who visited Rouffignac and described it (for instance Barry's name, who visited the cave in 1759, is superimposed on some profiles of mammoths). But it is not necessary to go so far back; I have personally had the assurance of M. de Laurière from Orléans that he had seen the great rhinoceros and mammoth frieze in 1938; he has recognized them with certainty whilst visiting the cave with me. I do not see why M. de Laurière's evidence should not bear the same weight as M. Pierret's.

In brief, it seems incautious, to say the least, to condemn the Rouffignac paintings on evidence which appears to me rather suspicious. The archaeologists who have examined the problem of the authenticity of the Rouffignac paintings have followed the right course in basing their judgment only on the objective examination of them, without letting themselves be influenced by considerations not pertaining to the paintings themselves. They have reached the following conclusions:

(1) Not all the paintings and engravings in Rouffignac are as bad and lifeless as it has been said. There surely are some which are, I should say, rather unusual and harsher in design than those of the most beautiful Franco-Cantabrian art. There are others, though, which reproduce nature with startling reality and which are executed with the same ability

² But are they not? Ed.

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of design recognizable in the classic paintings of other French caves. The works of art found at Rouffignac, in particular the paintings, are to be referred, in my opinion, to one of the last phases of the Magdalenian, the last period of the Palaeolithic; therefore one can already note a certain decadence in style, even if the basic characteristics remain unaltered.

(2) None of the archaeologists who have upheld the authenticity of the Palaeolithic paintings of Rouffignac has ever dreamt of considering *all* that is drawn in the cave as belonging to that period. I have already stated very clearly myself that there are in Rouffignac, as well as in many other painted caves of France and Spain, together with the modern inscriptions, some non-Palaeolithic drawings, undoubtedly modern, which it seems to me are very well distinguishable from the ancient ones. This in no way means that the fundamental core of the works of art of Rouffignac are a fake or that there are in this cave some attempts at falsification: with this I mean attempts to mislead the good faith of the general public.

I could here elaborate on what I have said on various occasions regarding the careful and meticulous examination which I have made on the physical state of the paintings and engravings in Rouffignac, and the data collected by me which exclude the modernity of those figures. In particular I have studied their relationship to recent engravings, or engravings of a few centuries ago, the reaction of the manganese of the black paintings to local conditions in various sections of the cave and the extremely ancient alterations to which the engravings have been subjected, which are the same as those found on the rock surface. (See *Rivista di Scienze Preistoriche*, XII, 1952, 117-122.)

BRITISH SCHOOL OF ARCHAEOLOGY AND HISTORY IN EAST AFRICA

We welcome the very good news that the Treasury, through Parliament, has agreed to the establishment of a British School of History and Archaeology in East Africa, financed in part by non-Government sources but with a substantial Treasury subsidy. It is designed to attract and foster research in the past of Kenya, Uganda, Tanganyika, British Somaliland and Zanzibar. Tanganyika has made a provisional offer of substantial premises at the old coastal capital of Bagamoyo, north of Dar-es-Salaam.

In describing the foundation of this new British school in his farewell address as President of the Society of Antiquaries of London on 23 April, Sir Mortimer Wheeler also revealed that a committee had just been set up by the Universities of Oxford, Cambridge and London, in consultation with the British Academy, to consider the establishment of a British Institute in the Far East—perhaps in Peking, but probably in Tokio. We wish all success to this important and necessary project.

Reviews

GESCHICHTE DER NEOLITHISCHEN STÄMME IM EUROPÄISCHEN TEIL
DER UdSSR. By A. JA. BRJUSSOW. *Akademie-Verlag, Berlin*, 1957. pp. 322, 68 figures.

Professor Brjussow's book on the history of the Neolithic tribes in the European part of the Soviet Union (already familiar to us from, among other things, its incorporation into the latest edition of Childe's *The Dawn of European Civilization*, 1957) has now appeared translated into German, making it accessible to wider circles of Western investigators. No matter how we may view the political situation that has brought such a work within the bounds of possibility, it must be hailed as an important scientific event that we are now able personally to form an opinion, not only of a valuable work by an eminent archaeologist but also of the train of thought and working methods of Soviet colleagues.

It was not the author's intention to present a manual of Neolithic cultures, but to essay an interpretation of the social and economic history of the Neolithic tribes throughout the vast region forming the European U.S.S.R. of our day (the title of the translation, by the way, is rather more categorical than that of the original work: *Očerki po istorii plemen evropejskoj časti SSSR v neolitičeskijuepokhu*). All the same, as a background the author describes most of the Neolithic cultures, but not to anything like the same extent. Central and North Russia's comb and pitted ware cultures (in the widest sense of the word) are examined and illustrated so exhaustively as to make it the best and latest survey of that field in any Western European language. There too the author of course has done important work as a primary investigator. What in large survey works are usually termed 'Survivals of the Forest Culture' in Eastern Europe now appear subdivided clearly into a number of independent archaeological cultures. Within European U.S.S.R. the principal ones are: the Oka culture (around the valleys of the Oka and Kliasma in the Moscow area), the Kargopol culture more to the north, the Carelian culture north and north-west of Kargopol (from Leningrad across lakes Ladoga and Onega to the coast of the Gulf of Onega), and farthest north the White Sea culture. The Karelian culture is associated with the Finnish Comb Pottery culture, but this is outside the scope of the book. Then east of the Oka tribes we have a new group in the Middle Urals and related trans-Uralian cultures. And finally, farthest south-west there is a Baltic culture region. Several of the cultures already have distinct subdivisions; for example, the Oka culture is split up into five groups with a hint of a sixth group around Tulá.

All the above cultures must be attributed to hunters and fishers, but they are described as Neolithic. Accordingly it is important to know that the concept of 'Neolithicism' in this context differs somewhat in form from most recent archaeological works dealing with other parts of Europe. The author describes a culture as Neolithic when, on the one hand, it has no metal-working of its own, but on the other hand makes use of raw materials and special implements that are unknown to the true Palaeolithic and 'Epi-Palaeolithic' cultures. The Neolithic people proper were also acquainted with pottery; but in almost all regions one encounters a number of earlier Neolithic cultures devoid of that element; these are generally called 'Early Neolithic'. Elsewhere many of the same groups would be described as Mesolithic. Thus the border between the concepts 'Epi-Palaeolithic' and 'Early Neolithic' is rather vague in several places.

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The Early Neolithic cultures are dated to something before or after 3000 B.C., whereas the fully Neolithic, pottery-using cultures extend from the 3rd to the 1st millennium. These datings are submitted with caution and, as a rule, are quite unstable in their foundations. Pollen-analytical datings are few (and most uncertain), and only exceptionally is there any possibility of making contact with southern cultures.

There are still astonishingly few early finds capable of providing information as to the origin of the Neolithic cultures. Apart from genuine Palaeolithic cultures the author can quote only four find-spots within the whole region which have yielded bone artifacts of those types which are often described by names such as 'Maglemose-like' or 'Maglemose tradition' (Schigir). The author holds the pronounced opinion that these finds (perhaps dating back to Boreal times) did not derive from the Maglemose culture to the west, but are evidence of an earlier settlement of the region from east and south-east, i.e. from the Ural regions. At all events it must be clear that there is no question of true Maglemose culture in the U.S.S.R., and that the Baltic Kunda culture is related more closely to the Eastern than to the Western settlement finds.

An entirely new form of culture was brought along by the Fatyanovo people to the territory of the Oka tribes at some time towards the close of the 3rd millennium. These new people were cattle breeders and, according to the author, cannot have derived from the ancient hunting people in the same regions. Their appearance signified an immigration of new people who displaced the original population. Unfortunately the Fatyanovo culture is not described so intimately as those referred to above. (We now have A. Häusler's detailed survey in *Arbeiten aus d. Inst. f. Vor- und Frühgeschichte der Univ. Halle*, vol. 5 (1955)).

Something similar may be said of all the large Neolithic cultures in the southern forest region and the steppes. The descriptions of Tripolye, the so-called Volhynian Megalith culture, the Middle Dneiper culture, shaft-grave and catacomb cultures are all brief. The author refers to monographs (in Russian) and deals solely with their forms of occupation (as to which very little can be said in many cases—apart from Tripolye, of course), and simultaneously he argues energetically on all sides in matters concerning the varied opinions held. The author's theory is that the growth and expansion of a culture may proceed in several ways, sometimes by internal development and 'segmentation', and sometimes by migration and conquest. To most Western archaeologists this is a long recognized idea, but it must be remembered that until 1950 the latter possibility was unknown among Soviet research workers, one and all of whom pursued the Marr culture theories of stadial development. As is known, the change came with a Stalin paper in which these theories of gradual, autochthonous evolution within all cultures were declared to be anti-Marxist. The polemic against the opinions of Soviet colleagues of pre-1950 date must be understood against that background.

The cudgels are also wielded forcibly against West European scientists, Kossinna especially. Now of course it is no longer the principles that are discussed but the direction of the various migrations and cultural connections. Here it must be said with all due respect that Kossinna's 'Indogermanic' migrations have been regarded with deep mistrust for many years by the majority of Western archaeologists. In the matter of the Volhynian culture and its association with the Globular Amphorae culture there is no reason for attaching such importance to the differences, considering that both groups may well be regarded as late local developments of the TRB culture (Trichterbecherkultur), i.e. that they had a mutual origin. (By the way, from the references it is observed that outside of the U.S.S.R. the author confuses the Early TRB culture and the Globular Amphorae culture. He makes no mention of the former, although in its pure form it occurs in the westernmost part of the Ukraine). On the whole it is possible to discern a decreasing certainty in the

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author's theories the farther he gets from the Central and North Russian culture groups; on reaching the borders of the region described and west of them he lays himself open to many objections.

The positive aspects of the book are unaffected by things such as these. This is the first time any attempt has been made in Soviet Russia to correlate the history of the Neolithic cultures, and the author himself is aware that further work is likely to make changes on important points. For instance, it is obvious that the Early Neolithic cultures throughout the southern U.S.S.R. are hardly known at all as yet; there is both chronological and geographical space for more culture groups.

C. J. BECKER

THE MEGALITH BUILDERS OF WESTERN EUROPE. By GLYN DANIEL. *London, Hutchinson, 1958. (Hutchinson University Library.) 18s. pp. 142, 25 figures, 8 plates.*

There are enormous numbers of megalithic tombs in Western Europe. These numbers are at once the hope and the despair of prehistorians: the hope because a thorough account of so many easily recognizable sites should provide a framework for events at a vital period of development while Atlantic Europe was emerging from the Mesolithic into a full Bronze Age; the despair because the adequate survey and record of so many sites so far-flung in distribution is a slow and tedious process which must inevitably proceed unevenly from region to region. As work progresses, reviews of the whole field are essential if we are not to lose ourselves in local details. Such reviews, of necessity tentative, are bound to leave those who attempt them open to criticism. It is well-nigh impossible in the present state of work adequately to keep in touch with the developments throughout the region, and in attempting a synthesis at this stage, one's generalizations are bound to fall foul of the particular. Dr Daniel's account in his *Dual Nature* paper of 1941, taken in conjunction with the work of Forde and others, helped greatly to marshal the scattered evidence and, though detail after detail has since—even with justice—been controverted, was a major contribution to Megalithic studies. In his present book once more he reviews the whole field, and for his courage in so doing again merits our best thanks.

The book falls into three sections. The first two chapters, comprising about one-third of the whole work, are devoted to introductory material. Having discussed megalithic architecture in general Dr Daniel describes the main features of the tombs. Included in the discussion are circles, alignments and standing stones, but these fit uneasily into the picture. The general reader will find these chapters instructive and even entertaining. He is warned by the author that much of the classification is to be taken as descriptive nomenclature rather than as strict typological reality. Interesting little tables surveying the occurrence of types are given. Closer examination of these is not altogether rewarding. Surely, for instance, the undifferentiated passage graves can hardly be described as restricted in their distribution, and the evidence for cairn shape on wedge-shaped galleries speaks at least as much for round as for long cairns. One might take issue with Dr Daniel on the question of earthfast sites and freestanding tombs. Certainly his remarks on freestanding chambers are not strengthened by his instancing of the Burren tombs, since clear traces of approximately round cairns are present in many cases.

The monumental work of the Leisners in Iberia has scarcely been availed of to the full. In turning to France, an area of which Dr Daniel himself has for long made a special study, the present reviewer at least was disappointed. Too many old friends (or should one say enemies?) turn up again. La Halliade, the Grotte de Fées, Tressé, etc. A few new plans would have been welcome. One suspects that the real good wine is being kept for Dr

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Daniel's forthcoming *Prehistoric Chamber Tombs of France*. In dealing with Britain and Ireland there are many changes from the 1941 approach. Unfortunately, Tibradden still holds an important place among Irish passage graves. Even if the structure were a real megalithic tomb, its extraordinary differences from normal Irish passage graves should have acted as a warning against over-emphasizing its rôle in the Irish series. The suggested derivation of the Portal Dolmen from the Wedge is not borne out by the form of the tombs, their distribution, nor their grave goods, though these last are still scant. Incidentally, Dr Daniel frowns on the term dolmen and would prefer to substitute for Portal Dolmen the almost equally ambiguous periphrasis 'Single Rectangular Chambers with Portals'. He is kind enough, however, to permit the use of Portal Dolmen for those 'for whom the word dolmen has no fears and implications'.

In a short concluding chapter, Dr Daniel poses three main hypotheses. In doing so he indicates his virtual abandonment of much of his Dual Nature thesis, and proposes at least four separate independent places of origin within Europe. This is hardly necessary. The notion that some groups are to be explained as indicating the spread of the custom of Megalithic building outside of the main core of what we may still call the great Western European Passage Grave complex has something to recommend it, and an explanation of the Severn-Cotswold tombs and the Court Cairns in such terms may yet prove to be correct.

While the specialist will undoubtedly see much to criticize and may be impatient of the large amount of historical matter, he will find much to stimulate him in his work. The general reader will study Dr Daniel's account, presented in an easily available and attractive form, with pleasure and profit.

R. DE VALERA

AN ARCHAEOLOGY OF SOUTH-EAST ENGLAND. By GORDON J. COPLEY. *Phoenix House, London, 1958. pp. 324, 20 figures, 22 maps, 28 plates.*

It is a pity that Crawford did not live to see and to review this book, for he would have appreciated it as a first-class example of the application of the 'Crawford Method' to a region of England. Mr Copley has essentially written a field archaeology and, although he is careful to set the monuments he describes into their appropriate framework of prehistory or history, there are no illustrations of archaeological material other than sites and structures. This is perhaps a pity, as it might lead to an under-estimate of the importance of museum material in the mind of the amateur, but if he reads the book with care he should not fall into this trap. In his first chapter Mr Copley defines the status of the amateur in British archaeology in admirable terms, making it clear that academic standards are involved, and firmly saying, 'At whatever level one works there can be no escape from the strict discipline that prehistory demands'—and, one might add, history also. For the book is not only concerned with prehistory, but includes valuable chapters on the field archaeology of Saxon and medieval England and on the use of place-name evidence.

The illustrations are of unequal quality. Many of the site plans have been roughly and badly redrawn (e.g. figs. 4, 7, 13, 14, 18), and the set of Regional Maps (A–L) all suffer from having been prepared by a draughtsman insufficiently aware of the results of reduction for block-making, so that balance is bad, lettering difficult to read, and symbols sometimes indistinguishable. The period distribution maps, nos. 1–10, are much more satisfactory. The plates are good and, in addition to some inevitable old friends, contain other less familiar subjects.

Mr Copley is to be congratulated on his book, which, apart from any other results, should increase a public awareness of ancient monuments and the threats to their destruction in

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the region described. One is prompted to wonder once again why such books should almost invariably be produced by field archaeologists in southern England while great tracts of Britain remain without comparable surveys. Differing patterns of social geography must have something to do with it: the distribution of potential readers of such books not less than of potential writers is uneven, and the high-standard amateur for whom Mr Copley has written seems alarmingly rare in some areas. Regionalism is all very well, but let us hope that in this respect we may see before long a more uniform spread of such workers, and that writers will be ready to present them with regional surveys as useful as Mr Copley's.

STUART PIGGOTT

THE ETRUSCANS. Vol. 7 in the series 'Ancient Peoples and Places'. By RAYMOND BLOCH. *Thames and Hudson*, 1958. pp. 260, with 79 plate figures and 41 figures in the text. 25s.

This is a good book for the general reader. It is written by a scholar who has both field experience and a sound understanding of the artistic, historical and religious background to the civilization he is studying. This lends the book a balance which is important in a work of popular interest and with so wide a subject. Etruscology has long flourished. The Romans themselves were curious about the people they had overthrown and assimilated; the Renaissance artists were not unaware of their works although it took long for them to be properly distinguished from native Greek; and today the enigmas of their origins and language can excite both scholarly controversy and the lively interest of a wider public. Their art too has become better known from many recent picture books and the fine exhibition which toured Europe recently but never reached Britain. Bloch is especially good at describing the background to the study of the Etruscans, defining the limits of what is and can be known about them, and summarizing current theories about their language and original home. The last is probably the most intriguing of the problems. The theory that they were intruders from the north is rightly discredited. The indigenous character of much of their culture is clear, but direct literary evidence and the strong circumstantial evidence of archaeology show that it should have been the arrival of immigrants from somewhere near the seaboard of Asia Minor which led to the rise of a distinctive 'Etruscan' civilization among the Italic peoples. But even this may be too easy an answer, and it is possible to underestimate the importance of the nearby Greek colonies in the Hellenization of Etruria. This scene-setting occupies the first third of Bloch's book. The second deals with Etruscan history, political and social. The last with religion and art. In these the author is generally well served by his translator and publisher. The maps and plate illustrations are good although some black backgrounds might have been avoided. The choice of illustration follows too closely the pattern of Bloch's earlier popular books on the Etruscans in French, this being a revised and expanded version of his latest, *Le mystère étrusque*; but there is much to please even if we miss some favourites. Many of the line drawings in the text also derive from Bloch's earlier books. Their cursory execution does scant justice to the archaic precision of Etruscan art and their placing in the text is often irrelevant, sometimes quaint (p. 111, a nursing mother to illustrate Roman military expansion). References to illustrations are also sometimes rather arbitrary.

The student, for whom this series is also intended, may be better served elsewhere although the book may readily stimulate interest where there was none before. Too many mistakes which could have been eliminated vitiate its value for serious study. A devil seems to have got into the works at some points, from the Table of Contents with two chapters beginning on the same page, through many minor misprints (centuries for millennia on

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p. 61), to the Notes on the Plates, where five entries are badly mixed up (pls. 24, 27, 28-30; 27 is from the Tomb of the Funeral Couch). For other shortcomings some less supernatural agency must be held responsible: the translator perhaps for the 'palm-leaf' palmette (p. 177) and the red vases (for red-figured); the author for the Notes on the Plates, which read like an auction sale catalogue, for thinking that Apollo and Herakles were just out hunting when they fought over the doe (pp. 154, 249), for an Ionian confederacy centred at Ephesus (p. 116), and (a prize howler) an 'Etruscan Zeus' at Olympia (p. 108) who is conjured from a mistranslation of the inscription on Hiero's famous helmet dedication. For the last the author—a student of omens and prodigies, as the dust-cover tells us—may well expect thunder in the South-West.

JOHN BOARDMAN

SYRACUSE: A HANDBOOK TO ITS HISTORY AND PRINCIPAL MONUMENTS.

By MARGARET GUIDO. *Max Parrish, London, 1958. pp. 96, 12 plates, 13 figures and 2 maps. Price 10s. 6d.*

Although technically a guide-book, the word may be used of Mrs Guido's *Syracuse* without any of the dreary undertones usually associated with it. Indeed, Mrs Guido has admirably succeeded in writing a short history of the city and a description of its monuments with the authority which her scholarship would lead one to expect, without disdaining to tell her reader the numbers of relevant buses to the various sites, where he may buy an English newspaper, and where dine after a day of sight-seeing.

The historical outline at the beginning of the book is, of course, intended for the non-specialist, but many a specialist will be grateful to Mrs Guido for information on events outside his own sphere of study. The description of the monuments is always lucid, a great boon, particularly in the case of highly complicated buildings like the Greek Theatre and the Roman Amphitheatre, of which the photographs and plans are closely enough related to each other and to the text to make the description easy to follow. Her guide to the Museum also will be appreciated by those who may have little time to spare for searching all its thirty-three rooms for objects in which they may be particularly interested.

The very useful items under the heading 'Some Practical Information' may need revision from time to time, and the opportunity can then be taken of correcting two minor slips in proof-reading which occur in the transliteration of the Greek inscription on p. 14, and at the end of the first paragraph on p. 64, where part of a sentence appears to be missing.

MICHAEL GOUGH

AN ANGLO-SAXON AND CELTIC BIBLIOGRAPHY (450-1087). By WILFRID BONSER. *Blackwell, Oxford, 1957. pp. xxxvii + 574 and viii + 123. Price £8 8s.*

Dr Bonser, Librarian to the University of Birmingham from 1929 till 1952, is well known as a folklorist, less well known than he should be as the author of an excellent paper on 'Survivals of Paganism in Anglo-Saxon England' in the *Birmingham Archaeological Society Transactions*, LVI (1932), 37-70. Much labour has gone to the making of this book, which is organized under twelve main headings: General Topics and Historical Source Material; Political History; Local History; Constitutional History and Law; Social and Economic History; Ecclesiastical History and Religion; Geography and Place-names; General Culture; Archaeology; Numismatics and Seals, Epigraphy; Art. The second volume comprises first an Author Index and then a Subject and Topographical Index. I do not wish to underrate Dr Bonser's positive achievement when I say that it is neither sufficiently selective nor yet comprehensive. A minority of worthless, or near worthless, items are included, such as Eminson's on Lincolnshire place-names, and No. 6732, which

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derives the county-name Somerset from *saw mor sedd* (' hunt-sea-abode '). No. 9508 should be expunged, for the stone found in or below the foundations of the church porch at Brough, Westmorland, and now in the Fitzwilliam Museum at Cambridge, is not runic at all but in Greek, being in memory of a boy called Hermes of Commagene; for this see G. F. Browne, *The Recollections of a Bishop* (London, 1915), pp. 203-7. On the other hand, many works of importance are omitted, sometimes when the *Annual Bibliography of English Language and Literature* would have supplied the missing item. *Festschriften*, too, for foreign scholars, might have been examined for relevant material. Thus there is no entry of any of the valuable papers in *Anglica* (in honour of Alois Brandl, 1925), to which Ekwall, Förster, Hoops, Keller, Klaeber, Luick and Mawer all contributed.

In general, Dr Bonser, as befits a pupil of W. P. Ker and R. W. Chambers, is stronger on the Anglo-Saxon than on the Celtic side. Thus of the few satisfactory works on Welsh place-names three of the best are omitted, Sir Ivor Williams's *Enwau lleoedd* (Liverpool, 1945), R. J. Thomas's first volume of *Enwau afonydd a nentydd* (Cardiff, 1938), and ' Parish Names in Monmouthshire ' in the unhappily shortlived *Monmouthshire Review* (1933-4), i, 386-409, and ii, 51-77. Nor is, for example, Hugh Williams's translation of Gildas in the *Cymmrodorion Record Series*, no. 3 (London, 1899-1901) to be found. There are also omissions in the Epigraphy section, as the elaborate account of the Ruthwell Cross in the Dumfries volume (1920) published by the Royal Commission for Ancient and Historical Monuments of Scotland, and that on the Maeshowe inscriptions in the corresponding volume on Orkney (1946). Nor is there an entry for the Mortain Chismale, the most important recent addition to the body of English runic inscriptions; see Maurice Cahen and Magnus Olsen, *L'inscription runique du coffret de Mortain* (Paris, 1930). But this is no place in which to list more than these few significant omissions. In conclusion, the rare faults escaped in the printing are for the most part in personal names and easily understandable.

BRUCE DICKINS

WESSEX BEFORE THE CELTS. By J. F. S. STONE. *Thames and Hudson*, 1958. pp. 207, 72 plates, 17 drawings, 5 maps.

THE ARCHAEOLOGY OF WESSEX. By L. V. GRINSELL. *Methuen & Co.*, 1958. pp. xv + 384, 15 plates, 18 drawings, 6 maps.

The prehistoric antiquities and field monuments of Wessex are among the most remarkable examples of man's handiwork in Western Europe. People rightly continue to be amazed at places such as Stonehenge and Avebury, while the more discerning note the significant differences in shape of our round barrows which cluster around the great temples of their day in cemeteries unmatched elsewhere. Hill-forts like Maiden Castle and Eggardun, Yarnbury and Hod make us wonder at the urgency which impelled such feats of military engineering. In the museums at Devizes, Salisbury and Dorchester, and at the Pitt-Rivers Museum, Farnham, there are antiquities which match these places in splendour and which have provided much of the chronological framework for the prehistory of central and western Europe.

It is surprising, therefore, that these two books are the first authoritative yet popular accounts to be written about the succession of cultures which have left their imprint upon the gentle landscape of Wessex. The late Dr J. F. S. Stone has written his contribution for the ' Ancient Peoples and Places ' series, and concentrates upon the pre-Iron Age periods; the typescript, mercifully, was drafted before his untimely death. Mr Grinsell has condensed many years of fieldwork and museum study into his book, which is bigger and broader in scope; it takes us down to the pagan Saxons.

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The authors approached the same subject in different ways and both have achieved what they set out to do. Stone has concerned himself with cultures and artifacts and a few sites with which he was especially familiar. The first farmers and their long barrows, Beaker/Battle-axe folk and individual burial beneath round barrows, the Wessex culture with emphasis on its exotic elements—and Stonehenge itself, these are his primary concern. He adds discerning comments upon the more insular elements in our later Neolithic (Durrington Walls and the ritual pits around Amesbury were very much his sites) and upon those Late Bronze Age farmers and herdsmen who marked the earliest surviving fields upon the downlands of this fabulous region.

Grinsell is one of the most industrious field-workers in British archaeology. Into his book there has gone almost a lifetime of tramping and measuring, peering into dusty show-cases and collecting folk-lore and delightful extraneous information. While Stone has concentrated upon cultures and their museum relics, Grinsell has devoted himself to the field monuments which successive peoples built in Wessex—long barrows and causewayed camps, henges and circles, round barrows, ditches, field-systems and hill-forts. Indeed, he carries us beyond the Roman conquest, sketching in the Saxon penetration of Wessex.

Grinsell's book is more a field guide than a textbook: it includes a gazeteer of nearly 1000 sites, classified and provided with two-figure map references, and ends with a list of museums in Britain containing things from Wessex.

Both books have admirable bibliographies and are adequately indexed. Grinsell has included a short glossary of technical terms (where, in desperation, he defines 'ritual pit' as any pit the purpose of which is not understood by archaeologists).

The plates in Stone's book are quite admirable, not least because most of them are new. It is unfortunate that scales are not printed beside the objects. The Rillaton cup is reproduced in colour, the Upton Lovell 'Bone barrow' has its grave-goods reproduced for the first time in near entirety; and it is interesting to see what the Inkpen footed bowl and beaker really look like. The grave goods from the Clandon and Ridgeway No. 7 barrows (Dorset) are properly illustrated at last, and the traces of scabbards on some ogival daggers from Wiltshire will be new to many people. Unfortunately, the gold-plated belt hook and eye-hole (for that is what they are) from Bush Barrow (Wilts) are shown on end instead of sideways.

Grinsell includes a series of aerial photographs, some taken by a newcomer to this art, E. Allen White. It is unfortunate that most of these views have been published before while sites like the Knowlton Circles, Eggardun hill-fort, the Normanton barrow cemetery and the enormous henge monument and saucer-barrow at Marden (Wilts.) continue to be ignored.

With so much material to consider, only a few comments can be made here. In *Wessex Before the Celts* there is no justification whatsoever for postulating an early (Windmill Hill) date for the erection of the Heel Stone at Stonehenge. The presence of one probable Windmill Hill potsherd and ten long barrows on the surrounding downs is really not adequate. It may be suggested, too, that the Mycenaean element in the Wessex culture is overstressed. If the dagger carving at Stonehenge could be seen for what it is—a British ogival dagger (like that shown in plate 49 in Stone's book) in its scabbard, the tendency might be halted. Stone's Mycenaean trading post at Amesbury could then be ignored, the handful of imported objects seen as trade gifts and the many native elements in the culture brought to the surface again. Although the 'Founder's Grave' theory is an attractive one, it lacks support from the barrow cemeteries themselves. It is not possible to establish Bush Barrow as the earliest mound in the Normanton cemetery (there are elements in this grave which suggest that it is late in the Wessex series), nor, for instance, is there any obviously

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early barrow at Snail Down. The association of long barrows with Bronze Age cemeteries is probably fortuitous, rising ground being chosen by most prehistoric barrow builders.

To continue to claim, as both authors do, that the causewayed camps were dug specifically for rounding up and slaughtering cattle and were never permanently occupied seems to be a denial of the facts. Evidence for pole-axing is very slight indeed. Moreover, the ditches of causewayed camps are notoriously productive both of domestic rubbish and of the bones of cattle, sheep and pigs. If one explains the lack of occupation debris from the centre of such sites first by inadequate investigation and then by the disappearance of nearly two feet of chalk by erosion since Neolithic times, these places can more happily be interpreted as fortified villages. Indeed, traces of a pre-enclosure timber structure have recently been found at Windmill Hill, and one post socket was found in a trial trench behind the ramparts at Robin Hood Ball.

Criticisms like these, which are mainly differences of opinion, cannot detract from the value of these two books. Their authors have served the cause of archaeology in Wessex well and it is deeply to be regretted that Marcus Stone cannot be here to share the acclaim which he deserves.

NICHOLAS THOMAS

FROM MYCENAE TO HOMER. By T. B. L. WEBSTER. *Methuen, London, 1958. pp. xvi + 312. 30s.*

In this ingenious book Professor Webster attempts to infer the nature of the lost Greek poetry that must have preceded Homer, also its relation to the literature of the near-eastern palace cultures and to Mycenaean and Geometric art. In the course of this necessarily hazardous undertaking he presents a useful synthesis of recent specialized researches, both archaeological and linguistic, and adds some interesting speculations of his own. The acceptability of some of these is diminished by formal imprecisions in the assessment and correlation of the evidence; in particular, the unproved hypotheses of one page are often the axioms of the next. Thus, of 13th-century houses outside the walls at Mycenae, 'These houses *may very well* have been the houses of palace officials, rather than private houses in the normal sense, so that palace records *can be claimed* from Mycenae as well' (p. 7; my italics). Poseidon 'may perhaps be seen' in a Cypriot Mycenaean vase painting on p. 48 (cf. fig. 14, one of the many good photographs at the end of the book), but on p. 204 this same painting is simply 'the late Mycenaean picture of the chariot of Poseidon'. These are minor examples of a characteristic which is due in part at least to the compression of the writing and the bland liquidity of its style. The experienced reader will not be too worried; the general reader might be seriously misled on occasion, but in any case he will probably find it easier to begin with Sir Maurice Bowra's excellent *Homer and his Forerunners*.

On the Linear B tablets Webster does not attain the far from excessive circumspection of the decipherers. This is particularly noticeable in the treatment of divine names from Knossos and Pylos. Dionysus is accepted without qualms as a Mycenaean god (p. 50), and on p. 285 the King is described as making offerings 'to the "thrice-hero" and "the lord of the house"', representatives of the mighty dead, who continued to send blessings to his people' (!). Repeated statements such as 'we know also from the tablets that something like divine honours were paid to the King in Mycenaean Greece' (p. 35) seem to depend merely on the King having a *temenos*. Like practically everyone else, Webster automatically assumes that *all* the tablets were written in Greek; but so few have yielded Greek, even under exceptionally heavy pressure, that we should consider whether some of them were written in another language.

Many connections of poetry and culture are traced between the Mycenaean palaces and

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Ugarit, Alalakh and Mari. Some of these, at least, depend on minor, inevitable, and therefore quite insignificant resemblances; for example, no conclusion of this kind can be drawn from the facts that 'a guest must be offered a chair, food, and drink, whether Athirat visiting El in the Ugaritic *Baal* or Hermes visiting Kalypso in the *Odyssey*', or that 'In Knossos we have some evidence that the younger women were trained by the elder women.' Similarly the inevitable incidents of early warfare (a man is hit in the neck by an arrow, holds his shield away from him, or fights round a beached ship) are accepted as a significant link between Homeric descriptions and Geometric vase-paintings. Again, Webster enlarges on Cyrus Gordon's connection of the Ugaritic tale of Keret with the tale of Troy; but the siege of a city, the abduction of a princess, cannot have been unique events in the Mediterranean and Near East of the 2nd millennium B.C. More evidence is needed for the claim that a pre-Mycenean siege-poem was ancestor of the *Iliad*; likewise for the assumption that ancient traditions can only have been passed down in verse-form.

Chapter 5, 'The Collapse of Mycenaean Civilization and the Ionian Migration', is a competent survey of the complex evidence and a good antidote to the fashionable tendency to put the beginnings of the migration down into the 9th century B.C., on account of the limited amount of Protogeometric pottery found at Ionian sites (which are only now, however, being properly explored). Webster is sensible, too, on late objects in Homer—except perhaps on Nestor's cup, where like most deltophiles he is distracted by *di-pa* at Knossos and Pylos meaning not a cup but an amphora. He has much to say, following Risch, on late linguistic elements in Homer (notably unresolvable contractions, *-ou* genitives, ignored digammas), especially in descriptions of 8th-century weapons and tactics. These criteria are applied rather rigorously and without discussion of the possibility, in any particular instance, of post-Homeric (rhapsodic or Alexandrine) adjustment; nevertheless, they are useful. The discussion of the Aeolic element in the language of Homer is incomplete; and I find it difficult to accept the dogma that the Homeric formulaic style originated in court poetry and the repetitions of protocol (as exemplified at Ugarit) rather than in the necessities of free oral composition as demonstrated by Parry. Much is made of the symmetry of parts in the two Homeric poems, and many pages, not all of them easy to read, are devoted to the attempt to establish a close correlation with the detailed symmetries of Attic Geometric pottery. I feel that this theme has been greatly exaggerated in recent years. Certainly it is hard to believe that we can fruitfully ask 'what sort of demands the people who enjoyed this revolution in the highly prized art of the potter are likely to have made of their poets' (p. 200).

The author thinks that the *Iliad* and *Odyssey* were made for a festival, or separate festivals, in the second half of the 8th century, and that their author(s) trained a team of reciters and made some use of writing. He disagrees with some of the conclusions that D. L. Page in *The Homeric Odyssey* drew from the examination of the vocabulary of the two poems; and argues that many of the words or formulae listed as peculiar to either *Iliad* or *Odyssey* are proper to the subject-matter of the one poem and not of the other. This is a useful discussion. However, in contending that the words adduced by Page amount to only 3 per cent of the whole Homeric vocabulary, he ignores Page's claim—which is manifestly a just one—to be giving merely a sample. It seems to me that Page is right in saying that there *are* notable differences of language between the two poems, though Webster is right in pointing out that some of these differences, at least, are not necessarily significant. Page is also correct in supposing that the linguistic differences (which can be supported by certain other types of argument, notably the widely differing treatment of the gods), strongly suggest different authors, though Webster has good grounds for questioning the view that the *Odyssey* was composed in complete isolation from the *Iliad* and its immediate tradition.

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The more tenuous the evidence, the greater the precision needed in making inferences from it. In this important respect the author's ambitious project has certain undeniable limitations. In general, though, it is a learned book, on a very difficult subject, which it would be unwise to neglect and from which there is much to be learned.

G. S. KIRK

THE GOLDEN TRADE OF THE MOORS. By E. W. BOVILL. London: Oxford University Press, 1958. pp. vi, 281. 8 maps. 30s.

The Golden Trade of the Moors is the successor to its author's *Caravans of the Old Sahara*, published in 1933 and long out of print. Though some of the text of the earlier book reappears, *The Golden Trade* is substantially a new book, with rather more emphasis on the Barbary end of the trade. It is an important historical work covering a wide and little-known field. For those readers who have no knowledge of Saharan trade and exploration, or of the history of the Negro peoples of West Africa, this book will open the door to a new world, as *Caravans* did for me. They will read of the ancient state of Ghana, of the kingdom of Mali which in due course replaced it. One of the Mali kings was Mansa Musa who is shown in medieval drawings holding a gold nugget and who crossed the desert in 1324 on pilgrimage to Mecca, entering Cairo with five hundred slaves each carrying a golden staff. The kings of Mali gave place to the Songhai who were overcome in their turn when a Spanish eunuch led an army for the Sultan of Morocco across the desert to Gao in 1590, transporting with it over the wastes the first artillery to be seen on the Niger. The tale is carried on to the European explorations of the 19th century.

In the Middle Ages gold from the Sudan poured into Morocco. Already Herodotus had written of West African gold, describing the silent trade between Punic merchants and the tribes of the seashore. Arab writers from the 10th century onwards describe a similar system operating by a river in the interior; Portuguese sailors of the 15th century and modern explorers brought back tales of it. But the secret of where the gold mines lay was preserved through the centuries from Arab and European alike. Mr Bovill concludes that the main source of the gold was near the Upper Senegal. By poetic justice the gold-fields were largely worked out by the time that outsiders reached them.

It is perhaps appropriate in this journal to look more specially at the commerce of antiquity. Roman glass and pottery found by Professor Caputo in graves in Fezzan have provided ample corroboration of the trade between the Romans and the Garamantes, as readers of Wheeler's *Rome Beyond the Imperial Frontiers* will recall. There are, however, many problems left, such as those of transport and of the nature of the goods carried to the north.

Before the camel came into general use in the Sahara, at some time in the Roman period, other beasts of burden had to be used, and asses and cattle are capable of very great endurance. Furthermore, in periods when trans-desert trade has been important, care has always been taken to maintain the wells along the routes. During the campaign of Valerius Festus against the Garamantes in A.D. 70 the Romans opened up a new and shorter route to the south, which is thought by many to be the one across the waterless Hammada el-Hamra. This route involves a great strain on men and beasts, and Mr Bovill makes the interesting suggestion that what made its adoption possible was the use of camels by the Roman expeditionary force. This makes sense, but it has yet to be proven. If true, it goes far towards explaining the two peaceful Roman expeditions across the Sahara in the early 2nd century in conjunction with the King of the Garamantes.

Mr Bovill is equally bold in his discussion of the objects of trade. He thinks that the

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common assumption that they were 'gold, slaves, ivory, ostrich feathers' requires qualification. Ostrich feathers may obviously go out, because the ostrich was plentiful in North Africa; Negro slaves do not appear to have been widely owned in the Roman world; he doubts the importance of the gold trade because of Roman access to other, more easily available, sources; he also has reservations about the ivory trade, because, for one thing, Strabo and Pliny do not mention it. They wrote, however, before the elephant became extinct in North Africa, though Pliny VIII, 4 implies its growing rarity: 'an ample supply of tusks is now rarely obtained except from India'. But African traders would hardly have been willing to see all the trade in this valuable commodity go to India. Is it not then possible that one object of the 2nd-century expeditions to the Sudan was to put the trans-Saharan ivory trade on a sound basis, at a time when the North African supplies had dried up? This is only another hypothesis, yet it may be recalled that the symbol used to decorate the mosaic floor of the Sabratha offices in Ostia was an elephant, and that the people of Lepcis had a marble statue of an elephant in their main street.

Mr Bovill considers that the staple commodity in both Carthaginian and Roman times was the African carbuncle—a red stone—which is mentioned by both Strabo and Pliny and associated by them with the Garamantes. As yet no carbuncles have been found in Fezzan. There have been suggestions that the carbuncle was a kind of chalcedony (one or two Saharan finds of which are noted by Mr Bovill), but the chalcedony on which Roman intaglios are carved is usually greyish-purple. Perhaps the intensive geological exploration of the Sahara by the oil companies now going on may produce some further information. Mr Alan Wells, one of the geologists, tells me that he has found quite a lot of grey chalcedony in Fezzan, especially round about the southern part of the Hammada el-Hamra, but he does not appear to have been impressed by its beauty or its value. As for other stones, Al-Bakri (11th century) mentions agates, red, yellow and white, found near the Adrar of the Iforas south of the Hoggar, which were exported to Ghana; French geologists have recorded a deposit of the green stone called amazonite in N.E. Tibesti, which had been exploited over a long period. All this, however, does not seem enough to account for a large trade with the important coastal emporia of the Roman period. The carbuncles are still to seek.

As regards the Garamantes themselves, the view that they were Negroid (p. 34) is now probably to be rejected in the light of Professor Sergi's study of skeletons from graves near ancient Garama which suggests that the Garamantes were allied to the modern Tuareg.

OLWEN BROGAN

EXCAVATIONS AT CLAUSENTUM, SOUTHAMPTON, 1951-9. By M. AYLWIN COTTON and P. W. GATHERCOLE. *H.M. Stationery Office*, 1958, pp 169, pl. 6. £2 5s.

This is the second of the Ministry of Works' Archaeological Reports on excavations financed by the Ancient Monuments branch, carried out in this case on a Roman site threatened by building at Southampton. The Ministry, thanks to the initiative of its former Chief Inspector, the late Bryan O'Neil, now have a procedure by which all development schemes are submitted to their scrutiny: if an ancient site will be affected, they endeavour to adjust the project to avoid or minimize the damage and, if that is not possible, they arrange for an excavation to take place. Such excavations are usually entrusted to a free-lance archaeologist, backed up by the financial and other resources of the Ministry. Publication of the results is arranged with the local Archaeological Society, which gain a good report for their transactions at a small cost or, if of general importance, with a national

journal. From time to time, however, large-scale work has to be undertaken and an adequate report is too large for a periodical; it is then published as a monograph in a handsome format by the Stationery Office in this series. The only snag is that the Government machine tends to work slowly; Mrs Cotton completed her excavations in Southampton in 1951, Mr P. Gathercole supplemented them by a fortnight's work and a ten-page report in 1954 and it is 1958 before the results are published.

Let it be said at the outset that this is a good piece of work, competently carried out in the field, and the results well organized, illustrated and documented in the report. The excavations uncovered nothing that was spectacular, no mosaics, no 'buried treasure', no complete building, but from the bits and pieces that must suffice an archaeologist working in a built-up area, Mrs Cotton has put together an invaluable outline for the historical development of Clausentum.

The Roman settlement at Southampton lies in a loop of the Itchin at Bitterne, three miles up the river and away from the present city, centred since Saxon times at *Hamwih*. In the 1st and 2nd centuries, beginning shortly after rather than before A.D. 70, as Mrs Cotton's excavations have now shown, it was a port settlement spread over 27½ acres, with timber houses and wharves from which lead from the Mendip state mines was exported. Whether it began as an open settlement or enclosed by a rampart and ditch is uncertain, as the outer earthwork is still undated. An inner line of defence consisting of a remarkable stockade of close-set posts, 9 ft. wide, and a ditch was constructed in the second half of the 2nd century, enclosing 8 acres within the river bend, but went out of use before A.D. 180.

The late third and first half of the 4th century are a puzzling blank. Seven milestones built into the later town wall show that roads to the port were well maintained till the time of Aurelian (A.D. 270). Thereafter it seems probable to the reviewer that the Saxon menace to shipping led to the decay of the port and in the new scheme for coastal defence initiated by Carausius and developed by his successors, that Clausentum was passed over in favour of Portchester and Carisbrooke. Certainly the excavations offer no support for the notion that Carausius had a mint here, from which coins marked C.L. were issued.

Finally, about A.D. 370, as Dudley Waterman's excavations of 1937-8,¹ demonstrated, a 10 ft. wall was constructed along the northern river front, which Mrs Cotton has been able to correlate with some small-scale rebuilding in the town, and which she tentatively assigns to the work of Count Theodosius. The finds show that the place continued to be occupied till the beginning of the 5th century. Thus Clausentum has provided a conspectus of the history of a small port settlement, a type of Roman site about which little is known in Britain.

Much of the volume is taken up with an elaborate study of the pottery finds. Southern Britain badly needs what Mr J. Gillam has provided recently for the north,² a corpus of dated coarse-pottery types. Mrs Cotton has assembled a valuable series arranged in groups from dated levels (pp. 91-138), and also in a chronological sequence according to the types of ware (pp. 61-90), with full cross-references in each section. This thorough study is marred by the crowding of the illustrations into a small area on a spacious page (e.g. figs. 26-7, 29) which makes it difficult to appreciate the shapes and to single out individual pieces, let alone compare details of profiles on different pages. In comparison the well-known plain samian wares reproduced at half-scale on figs. 16-18 occupy unnecessary space.

AILEEN FOX

¹ *Antiquaries Journal*, xxvii, p. 151.

² *Arch. Aeliana*, xxxv (1957), p. 1.

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THE LOST GODS OF ENGLAND. By BRIAN BRANSTON. *Thames and Hudson*, 1957.
pp. 194. Price 25s. net.

Mr Branston is a daring scholar, and sometimes rash, and for these reasons his work is stimulating. In the present book, Mr Branston's aim is to reconstruct the religion of the pagan Anglo-Saxons. As is well known, the literary and archaeological sources of pagan beliefs and practices in Anglo-Saxon England give but a poor picture, but Mr Branston argues that this picture may be supplemented, if not completed, by the use of Scandinavian sources, particularly the Poetic Edda and Snorri's Edda.

In recent years, scholars have written with great caution about the pagan religion of England, feeling either that paganism was never strong, or else that the sources could not provide a clear conception of it. It is against such scholars that Mr Branston revolts, and, in particular, against such established authorities as Professor D. Whitelock, Sir Frank Stenton, R. Hodgkin (who is given the lie) and H. Shetelig, whose opinion is knocked on the head.

Opinions so revolutionary as those expressed by Mr Branston need to be backed by sound and exact scholarship. It cannot be said that they are. Words and names are often misinterpreted and thus false conceptions are developed. The name of Völundr's alien, valkyrie bride is given as 'All-White', apparently a misunderstanding of the word *alvitr* (cf. Old English *aekwiht*, 'strange creature'). The obscure Ánarr (or Ónarr), one of the husbands of Nótt (Night) is called by Mr Branston Annarr or 'the Second', as indeed he was in some editions of Snorri's Edda, although against strong evidence. The name of Night's father is given as Nökkvi, identified with *nokkvi* (ship), and said to imply that Night's father was helmsman of the moon. This might appear to be correct, but Snorri called the father of Night Nörfi or Narfi, and not Nökkvi. The names Naglfari and Auðr, occurring in the same passage of Snorri's Edda, are interpreted without hesitation as 'Twilight' and 'Space', although it is difficult to see why. Aurvandill, whose frozen toe was broken off by Þórr to become a star, is read as Orvandill. Presumably the first element is equated with *ör* (arrow),¹ and so Aurvandill comes to be identified with the archer Egill, brother of Völundr, who is depicted on the Franks Casket. The train of thought is difficult to follow.

Mr Branston often reads pagan concepts in or between the lines of Old English poetry. The *Frea* of *Beowulf* is, of course, associated with the god Freyr, as is the *Frea* of the Dream of the Rood. Evidence is said to show that the Anglo-Saxons knew the cult of Freyr and of his sister Freyja, although this evidence is hard to disentangle. Professor Whitelock is wrong in supposing that the flood, to which reference is made in *Beowulf*, is based on the biblical flood. It is rather to be identified with the flood in Norse myth, which drowned all the frost-giants but Bergelmir. Bergelmir is said to have escaped on or in a *lúðr*, which Mr Branston interprets without question as 'mill or mill-stand'. Whatever *lúðr* may mean it does not, in this context, mean a mill-stand.

Considerable space is occupied in attacking opinions which have rarely, if ever, been expressed. Few have doubted the eminence of Týr in early Germanic religion, and it seems unnecessary to argue for it so forcefully as is done in this book, or to credit Týr with so many of the achievements and sacrifices of Óðinn.

Mr Branston's English is sometimes odd and his terminology eccentric. Instead of 'Germanic' we have to read the indigestible 'North West European'.

¹ In another book (*Gods of the North*, 1955, pp. 246 and 255) Mr Branston gave the form Örvandill. The form Aurvandill is supported by most manuscripts and accepted by recent editors and commentators. Opinions on the meaning of the name are divided.

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Although not thoroughly worked out, Mr Branston's opinions have some accidental affinities with those of Professor G. Dumézil, whose work does not appear to have influenced him deeply. Like Professor Dumézil, Mr Branston emphasizes the similarities between Germanic and other Indo-European religious systems.

In conclusion, I would recommend experienced scholars to read this book, but I would not be justified in recommending it to beginners. It is alive and vigorous, but often misleading.

G. TURVILLE-PETRE

MYCENEAN POTTERY IN ITALY AND ADJACENT AREAS. By LORD WILLIAM TAYLOUR. *Cambridge University Press*, 1958, pp. xx + 204, 27 figures, 2 maps, 17 half-tone plates.

This is a very welcome addition to the regional studies of Mycenaean styles inaugurated by Dr Stubbings. The material is presented in great detail but in a highly digestible form, so that it does not matter in what order the reader uses the book, so clear are the cross-references from text to plates and vice-versa.

Chapter II describes in detail the Aegean pottery found at Ischia and on the Aeolian islands, and includes an up-to-date classification of the Lipari material communicated by its excavator Professor Bernabo Brea as follows: (1) Neolithic: characterized by (a) impressed pottery like that of Stentinello in Sicily; (b) painted pottery like that of Matera. (2) Neolithic: with (a) dark, coarse, burnished wares; or (b) pottery in the Capri style with red bands outlined in black on a yellow ground. (3) Neolithic: coarse wares, but the Capri style disappears and is replaced by painted fabrics with patterns based on the spiral meander group like those of Serra d'Alto. Obsidian abundant as in (1). (4) The painted pottery was replaced by red burnished wares with tubular handles as in Apulia. Copper working and flint arrowheads appear for the first time. (5) The Piano Conte culture with coarse black burnished pottery decorated with furrows or burnished stripes like the Γ wares of Thessaly. (6) The Piano Quartara culture (restricted to the Diana site on Lipari and to a site on Panarea, somewhat analogous to the Early Helladic culture in Greece). (7) The Early Bronze Age with the coarse pottery known as Capo Graziano ware and the earliest Mycenaean imports (early Mycenaean I). (8) Middle Bronze Age with a fair quantity of Mycenaean imported sherds. (9) and (10) End of the Late Bronze Age and Early Iron Age (termed Ausonian I and II by Brea) with only two Mycenaean or Sub-Mycenaean sherds. (11) Hellenic level of the Cnido-Rhodian colony. (12) Graeco-Roman level.

Of these sites Filicudi actually produced one Middle Helladic sherd and two transitional Mycenaean I.

Taylor identifies five sherds as L.M.I. imports from Crete into Lipari, but notes that certain Vapheio cup fragments, previously classified as Minoan, should be reckoned as Mycenaean I imports from the mainland.

Five or six matt painted sherds vaguely recalling Middle Helladic examples he terms the 'Local Matt-painted', though warning the reader that they might conceivably be Cycladic imports.

The third chapter discusses the Mycenaean vases from Sicily which were relatively well known, but here Taylor identified for the first time a Middle Helladic cup from Monte Sallia, and a local imitation of it apparently from the same group of graves, and the other imitations of it in the Syracuse Museum from various sites.

Mycenaean influences are shown not only by imports, but by local vases and bronzes with Mycenaean characteristics. The author concludes that 'it would appear that there was a

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steady stream of influence from the Aegean, starting in Middle Helladic times and continuing to the very end of the Mycenaean period. It flows with perceptible strength during the Mycenaean III A phases and gradually diminishes thereafter', and he notes signs of increased connections with Cyprus and Crete in the later stages.

Appendix A to this chapter discusses the Mycenaean sherd from Malta identified by Professor Evans.

In Chapter IV the Mycenaean material from Scoglio del Tonno outside Taranto is described in detail, and the connections with Rhodian and Cypriot wares stressed. The oval hut sites, the 146 vases and 750 sherds recorded by Quagliati seem to imply the existence of a Mycenaean colony here. I do not see why Taylour should object to Quagliati's 13th century date for a violin bow fibula, seeing that the stilted and flattened bow varieties are dated L.M. III B (1300-1230 B.C.), by Furumark.

In Chapter V Taylour reviews the Mycenaean material from the other Apulia sites and the 'Iapygian geometric' pottery from Coppa Nevigata and Borgo Nuovo; he rightly rejects Blakeway's suggestion that the former group was Cretan Protogeometric and claims that both are local styles, the Coppa Nevigata group influenced by Greek Protogeometric and the Borgo Nuovo (which he regards as the parent of the Peucetian and Daunian fabrics), by Greek Geometric wares.

Chapter VI reviews the scanty evidences from other parts of Italy with appendices on possible evidences from Sardinia and Spain. The author rightly rejects the four vases from Torcello and notes that the gem from Corneto or Orvieto came from a dealer, but why does he list the latter among his Campanian material?

In the last chapter the author gives his conclusions. The Mycenaean expansion to the Lipari isles seems to have been mainly in the Mycenaean I and II periods and to have declined in Mycenaean III, when perhaps Liparite obsidian was being replaced by Melian or bronze. In Sicily and elsewhere Mycenaean III A seems to have been the chief period of expansion, in Apulia especially from Rhodes. At Taranto the contacts continued right up to the 8th century B.C., to which may be dated the Protogeometric ware of Coppa Nevigata and the Geometric wares of Borgo Nuovo. To this fact perhaps may be related the appearance in the Aegean of certain Terramare features, such as the palstave mould from Mycenae, but the lanceolate spearheads from Kephallenia with a Terramare parallel seem to be Central European types (see V. G. Childe, *Proc. of Prehistoric Society*, 1948, p. 184, and fig. 2).

R. W. HUTCHINSON

CHINESE ART. By WILLIAM WILLETTS. *Penguin Books*, 1958. 2 vols. 7s. 6d. each. pp. xxxiv + 802. 64 plates, 110 figs., 6 tables and 7 maps.

There are two ways of handling the history of an art as complex and enduring as China's—to treat each great historical period as a cultural entity, or to unfold in turn the complete and separate development of each art. Mr Willetts has preferred a compromise in which one art is made the typical achievement of each period. Jade and bronze serve for the pre-Han period, lacquer and silk for the Han, sculpture for the Six Dynasties and T'ang, pottery for the T'ang, and painting and calligraphy for the Sung and Yüan. This plan works quite smoothly, except that a few Han rags hardly represent the Chinese achievement in textiles, and the arts of the Ming and Ch'ing, apart from references in the last chapter on architecture, are ignored.

This is the first general survey of Chinese art published in England for some time.

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It is admirably cheap, well produced, and, considering the price, generously illustrated. It is moreover a big book—over 800 pages—and no popular primer. It is presumably intended for the scholar: to tackle it, the intelligent layman would need a huge appetite for self-improvement. The historical introductions to each chapter are models of lucid exposition with here and there a verbal felicity or a personal point well made and stimulating. This excellent style helps to carry the reader through the immense chapters, but it is put under terrible strain. Mr Willetts denies himself only what he calls 'exclamatory' criticism. For the rest his readers will find him very exacting. He tends to 'climb o'er the house to unlock the little gate'. The chapter on textiles includes some forty pages on sericulture, the Cos silkworm, mulberries, hatching and rearing silkworms, filature, fibre engineering, looms, degumming and so on. With his gift for *précis* and a few judicious references to articles by specialists this could have been reduced to five pages. These dreadful facts leave the reader dispirited when he finally comes to the objects themselves.

The best chapters are those on pottery and architecture. In the former he marshalls all the material clearly and concisely, and, a true English orientalist, is moved to commend, even to exclaim. The chapter on architecture is a discursive and original essay, beautifully written and wholly delightful. The section on 'pagodas', however, needs revision: the Indian evidence has not been mastered. The most impressive and most disappointing chapter is that on painting and calligraphy. The first dozen pages are the best I have ever read on the subject. He enumerates the principles by which we attribute an oeuvre to a period or an artist in Europe, and decides that no Sung painting of consequence can measure up to these exacting standards. What, then, is the status of Sung painting? Here he fails the reader, avoiding the issue with long debates on perspective, pigments, brushes and ink—a substance apparently quite different from the stuff we put in fountain pens. He will discuss Wu's oeuvre, he says on page 602, more fully on pages 646–7. Alas, in these pages there is nothing to suggest there is an oeuvre. Let us hope that when Mr Willetts has had the opportunity to see these paintings he will complete this chapter in the critical spirit of the first pages. The least successful chapter is that on sculpture. Here will be found the *disjecta membra* of many books, and, I am afraid, not a few errors—some grave. (The continuous and consistent repetition of 'Karpadin' for 'Kapardin' is most irritating.)

This is, in short, a brilliant effort, and more worth reading perhaps than many a better book. Only it is too ambitious. The yield is good, but with cruel pruning would have been immeasurably heavier.

DOUGLAS BARRETT

PATTERN AND PURPOSE: A SURVEY OF EARLY CELTIC ART IN BRITAIN.

By SIR CYRIL FOX. *National Museum of Wales, Cardiff*, 1958. pp. 160, 83 text figures, 81 plates and 3 maps, one coloured. 45s.

The Council of the National Museum of Wales are to be congratulated on the bold, imaginative and public-spirited step they took when they invited Sir Cyril Fox to follow up his study of the Llyn Cerrig votive deposit (published by the Museum in 1946) by undertaking a wider survey of Early Iron Age art in Britain as a whole. He had already thought and written much on the subject and he had, as he tells us, a title already in his mind indicative of the dual quality of craftsmanship and art inherent in his material—*Pattern and Purpose*. Under this title the Museum has now published his work in a generous and attractive form and at a price within any serious student's reach: it is ironic, but perhaps not surprising, that this major contribution to British archaeology and art history should have been sponsored and produced in Cathays Park, and not in Bloomsbury.

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The book is organized with that clarity and conviction which one expects from the author. An introductory section which sketches the archaeological background and defines terms, is followed by a first part which runs in chronological limits from the mid-3rd century B.C. to the advent of the Christian era. Here in the earlier phase the author can work in terms of continental imports and a broad division into northern and southern schools of craftsmanship; from the beginning of the last century B.C. (his 'Secondary Phase') localized schools multiply to six regional styles at least. In the second part the complex material from the last generation B.C. to the Agricola campaigns is discussed under heads such as Personal Display (indoors and outdoors), the representation of man and animals, coinage and minor craftsmanship.

Sir Cyril has excluded much of the Scottish material by design—it 'presents a special problem', he feels, and he flatteringly leaves it in the hands of the reviewer. Would the latter be thought too embarrassingly an expatriate Englishman if he reiterated his conviction that, as with most Scottish archaeology, this is a particular instance of a general myth, once sedulously propagated north, and as credulously accepted south of the Border, that Scotland is not to be studied as the northern part of Britain, but as a Secret Commonwealth with at best laws of its own, at worst wholly and unpredictably lawless? The Celtic art of Scotland can, in fact, only be understood in terms of that of England, itself acting as an intermediary with Europe; so too must Ireland be largely considered, though here direct continental contacts render it a somewhat separate case.

For the English and Welsh (and some Scottish) material, however, Sir Cyril has presented a luminous and perceptive analysis of style and period, which advances us notably beyond the only previous general survey, that of Leeds of nearly thirty years ago. Inevitably much of this develops themes set out in previous studies by Sir Cyril, but there is much new as well, especially the full treatment of the ornament on the great electrum torc from Snettisham, and on those two national treasures, hitherto lacking stylistic analysis, the Battersea shield and the Waterloo Bridge helmet, ably supported by Mr Alan Sorrell's fine drawings.

A point of criticism that may be made is that the relationship between the works of art and craftsmanship studied, and the structure of Early Iron Age prehistory in Britain and on the Continent, is not established in anything but the broadest outline. Here we are reminded indeed of Jacobsthal's somewhat similar, but far more culpable and infuriating treatment in his *Early Celtic Art*. But Sir Cyril has now presented us with the relevant British material, interpreted and analysed in ordered form, so that new studies can now proceed with his own classic treatment acting at once as a firm foundation and as a stimulating incentive to further work in, for instance, the field of continental and British relationships in metal-working traditions and the movements of actual craftsmen. Our apparent 'Waldalgesheim' contacts pose an immediate problem in archaeological terms, as do such features as the derived 'bird-bridge' sword-scabbards (his fig. 24) in relation to Swiss schools of armourers.

There is a final aspect of Sir Cyril's book to be mentioned, and that is the elegance and clarity of its prose, set off by the racy bravura of his own drawings. We have recognized these qualities in all his work, of course, for thirty years or so, but their rarity calls for something more than tacit acceptance. In a literature which increasingly presents itself as an ill-ordered string of clichés and passages of technical jargon linked in flat, inept prose, we should give rather more than three cheers that Sir Cyril, in a characteristic phrase of his own once used to the reviewer in another context, can 'use the English language as an instrument of power and compass'.

STUART PIGGOTT

ANTIQUITY

TERMS USED IN ARCHAEOLOGY: A SHORT DICTIONARY. By CHRISTOPHER TRENT. *London, Phoenix House, 1959. pp. 62. 6s. 6d.*

There is, to use a reviewer's cliché, a long-felt need for a glossary and dictionary of archaeological terms in a generation that is increasingly and healthily interested in archaeology. This book fails in this so badly that readers must be warned here is no safe guide for them. The *Oxford English Dictionary* and the *Oxford Junior Encyclopedia* are already far better guides; admittedly this slim volume costs only seventy-eight pennies, but with errors and horrors on almost every page, the 240 entries are desperately dear—except as a quarry for solecisms, howlers and unbelievable misconceptions. Mr Trent thinks Altamira is in the Pyrenees, that palstaves can be made of stone, that the Pyramids are megalithic, that dene-holes 'were used as storehouses in the New Stone Age', and that Strépy is a 'culture and industry of the Old Stone Age'. Like the lady to whom Dr Johnson was so rude, Mr Trent will believe anything; witness his extraordinary entry on 'pit-dwellings', surely unthinkable in a post-Windle, post-Winbolt, post-Little Woodbury age, and his assertion that Belas Knap 'represents more perfectly than any other similar monument the appearance of a typical burial place of the New Stone Age'. What of Stoney Littleton, New Grange, Gavrinis and Maes Howe; all unmentioned in this book? It is no use listing sins of omission like crannog, rath, souterrain, fogou, ogham, wheel-house, Picts, linear earthworks, cursus, Food-Vessel, Passage Grave, Jericho—to mention a dozen that come immediately to mind. The book is damned by its sins of commission and should be withdrawn and replaced by a reputable work as soon as possible. In his preface the author says, 'He who hesitates is lost', and ends an astonishingly inaccurate entry under *Cromlech* with the words 'but there is much confusion'. It is extraordinary that anyone should be prepared to exhibit his hesitations, confusions and ignorance to the public in a book like this.

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